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An Extraordinary Case of Phlegmasia Dolens. Communicated by Dr. Henry Malcolm of Philadelphia.

THE following is an extract from a paper read before the Medical Society of the county of Columbia, state of New York, at their semi-annual meeting in the city of Hudson, which took place on June 2, 1811.

The puerperal swellings of the inferior extremities, or phlegmasia dolens, I have always observed, even in its mildest forms, to prove a tedious and distressing complaint, and peculiarly obstinate as to the control of medicine; but in this instance such a continued succession of multifarious symptoms occurred, and so infinitely did it transcend in its severe results every case of the kind that I had ever met with in practice, or found recorded or related by others, that I am induced to make it the subject of the present communication.

The object of the great and peculiar sufferings in this case was a respectable married woman of this city, aged 38 years, of a sta-

ture above the common size, rather corpulent, and of a vigorous constitution, who had invariably accustomed herself to an active, and occasionally, to a labourious life.

The circumstances that might have been remarkable during every period of her gestation I was not made acquainted with; at a very early stage of it I was consulted, and bled her for complaints incident to that period. Between the 4th and 5th month her husband called on me for advice respecting a very troublesome swelling of the labia pudendi, attended with difficulty and heat in the passage of her urine, when I prescribed bleeding, the local application of saturnine solution, and cooling laxatives; my personal attendance, however, was dispensed with, as I supposed from motives of delicacy. About a fortnight after this I was informed that my prescription had been attended with the desired relief, and that she now felt but little inconvenience from the complaint. Some time subsequent to the above occurrence I accidentally met her at a neighbour's house, when I noticed a dark bilious appearance of the countenance, and she informed me that she had never been so unwell in her former pregnancies; that her feet in particular swelled more than was common to her, under the same circumstances.

At the sixth month she sent for me, when I found her complaining of severe pains about the hips and back, which much disturbed her rest at nights. To relieve these symptoms I opened a vein at the arm and took 16 ounces of blood, and directed laxative medicines to preserve her bowels in a soluble state, with the occasional use of an opiate if necessary. On the 26th November last, at the full time, she took her labour, when, after a short and comparatively easy period, I delivered her of a healthy female child. Nothing material occurred until the eighth day after delivery, when she began to complain of pains in the calfs of both legs, more particularly the left, which soon became more remarkable in the right, changing to the left, and lastly shifting and fixing in the right leg, viz. on the 12th of December, four days after the attack, when following the course of the lymphatics on the inside of the leg and thigh, to the inguinal glands, these also became painful; the limb now was greatly swelled, its surface of

a glossy and deadly paleness, and was insufferably tender to the slightest touch or pressure. From the attack to this period, fomentations had been assiduously employed, with the occasional use of soap liniment, but with little or no benefit; the pain and swelling kept on increasing. On the 14th various other external applications were used, but with no better effect; in the evening of this day she was seized with a sharp fever, attended with a full and rapid pulse; 16 ounces of blood were taken from the arm; her fever abated after this loss of blood, the pulse became calmer and more natural, but nevertheless remained quick and generally feeble; the heat of her body was increased, the tongue white and clammy, and the countenance dejected, with a sallow paleness, and the body costive: the secretion of her milk here stopped, and did not return again, but the lochial discharge was not affected.

16th. Heat of the limb being very great and pain intense, linen cloths, moistened with saturnine solution, were applied over the whole limb; this procured some relief from the heat and pain.

Early in the morning of the 18th I was called up to my patient, when finding that every symptom had increased to an alarming and distressing degree, and a consultation being now proposed by her friends, in which I cheerfully acquiesced, doctors White and Tallman of this city were accordingly called in. After some conversation with these gentlemen, the following plan of treatment was agreed upon:—Tinc. digital. gtt. 10 to 12, two or three times a day, or according to the effect, with cooling nitrous drinks, and the application of camphorated oil with opium to the affected limb.

19th, 20th, 21st, and 22d.—Symptoms stationary, except that her pain increases; direct the pulv. ipecac. comp. to mitigate the pain. 23d.—Pain of the limb is very distressing, and heat considerable; direct a cooling febrifuge of the acetated kali and the opiate powder, as before. 24th and 25th.—Heat abated, is very weak, some nausea and want of appetite; direct an infusion of chamomile to be taken cold three or four times a day. Local treatment continued. 26th.—No material change, except that she complains of a suppression of urine; direct an infusion of flaxseed with spt. nit. dul. to be drank freely, and an emollient injec-

tion to be administered. 27th and 28th.—Is relieved from the suppression of urine; continue the nitrous drink, and give a light infusion of the bark, and as her pain is occasionally very great, the Dovers powder. 29th and 30th.—Pain, swelling, and heat of the limb is much increased; she has some fever; omit the bark infusion, and continue the cooling mixture; apply the saturnine solution alternately with the camphorated oil to the affected limb, and give the Dovers powder as usual.

Here it may be necessary to remark, that a costive state having attended from the commencement of the complaint, this symptom had been usually obviated by injections; but at this period her alvine evacuations being composed of large, round, and somewhat hardened balls, of such an uncommon size as to occasion great efforts and extreme pain in voiding them, we directed a cathartic of calomel, to be followed by an injection if necessary.

31st.—The calomel, with the aid of an injection, has operated copiously, bringing away more of the indurated and enlarged fæces; and in the whole a more liquid and copious discharge than had been hitherto effected by injections alone; there is now some mitigation of pain, and she is in other respects rather more comfortable; but at this time a large œdematous swelling affecting the foot, three punctures were made, and it mostly subsided with a gradual discharge of water from the punctured parts.

Jan. 1st and 2d.—Costiveness recurring; repeat the cathartic of calomel, and continue the same local remedies. 3d.—The cathartic of calomel not proving so effectual as before, repeat the dose to-day, and apply mercurial frictions to the inside of the thigh in the line of the lymphatics, indurated and painful swellings appearing in their course. 4th.—An œdematous swelling of the labia pudendi proving troublesome, was punctured, and relieved in the same manner as the foot; at this time also the inguinal glands swelled in clusters readily distinguishable to the touch; some days previous to this, several vesications of a disagreeable appearance, their contents being not unlike dirty water, had spread on the outer side of the foot of the affected limb; at this time there appeared also an effusion of florid looking blood on the top and centre of the foot; over this these vesicles had extended,

and running together burst, and soon after formed a hard crust or scab, which, quickly irritating, caused an inflammatory swelling of the foot; to this an emollient poultice was applied; the cathartic of calomel, with the aid of an injection, has procured a free evacuation from the bowels, which are now in a more soluble state; swelling and pain of the limb continue the same. 5th.—On removing the poultice, the scab or spot to which it had been applied yesterday, being about the size of a half dollar piece, was found perfectly livid and gangrenous. An epispastic (as recommended by Doctor Physick of Philadelphia) was applied to the mortified part, and embraced a portion of the sound parts; this was kept on for 24 hours, and when taken off we had the satisfaction to find that the unsound part had become considerably detached from the sound; the part was then covered with a paste of yeast and bark, and dressed over this with a strong digestive ointment, and the bark in substance, with wine, ordered to be given as frequently, and in such doses as the stomach would bear. 6th.—The separation progresses favourably, and there is a discharge, circumstances considered, of a tolerable looking pus; local treatment the same as yesterday, and the bark and wine continued internally.

7th.—The tumefactions of the limb now became so great as to require bandaging; the sound parts retract from the morbid on the foot, and the discharge of matter increases, the patient is extremely weak, pulse feeble, and she has a disposition to faint when in an erect posture; ordered the vol. alkali in small doses; continue the bark, &c. and the same local treatment. 8th.—The sound parts retract so much that a complete channel is formed around the mortified part, the discharge is considerable, and continues to mend in colour and consistence; the patient, however, is still weak, and pulse small; continue the same treatment, both general and local.

9th.—She is feverish, the pulse is quick, small, the stomach is much disturbed, she is comatous, with occasional alienations of mind, and there is great debility; directed a gentle emetic of ipecac. and after the operation to continue the same treatment as before.

10th, 11th, and 12th.—Complains now of pain on the spine of the tibia of the affected limb, which increases; febrile symptoms are declining, she is less comatous, pulse more calm and regular, strength better, and mind more composed; the ulcer necessarily enlarges around the mortified part, from the continued retraction of the sound: and here permit me to remark, that from such a long continued compression of the parts of the whole limb, they became in so torpid a state, that wherever the skin broke there appeared a disposition to form a spreading and ill-conditioned ulcer; no doubt too but that this tendency was increased by the morbid state of the system, and by the long confinement of the limb. 13th.—Febrile symptoms disappear; the bark and wine to be continued, and the ulcers dressed as usual. 14th.—With a little assistance of the knife a complete removal of the mortified part was now effected, when the flesh underneath appeared florid, with healthy looking granulations; there was also a considerable discharge of a favourable appearance, and the ulcer spreading to within an inch of the toes, and extending obliquely to about the same distance from behind and under the outer ankle joint, would now nearly admit a hand of common size. During this spreading progress of the ulcers, the tendons of all the small toes were destroyed back to the ring of the annul arligament of the ankle. During this progress there also appeared a small cranberry coloured spot on the inner ankle; this soon became gangrenous; an epispastic was applied to it, and the routine of treatment was pursued as for the same affection on the foot, and with the like good effects. This ulcer also arrived to about the size of the palm of the hand, threatening the capsular ligament of the joint. Foul aphthous sores next began to form in the mouth and throat; for this affection a gargle was directed composed of an infusion of nigella, with a few drops of the muriatic acid. Bark and wine continued, and the ulcers dressed as before.

15th.—Aphthæ of the mouth, &c. increases, and is very distressing to the hapless patient; various other detergent gargles were had recourse to, but with no benefit; ordered a decoction of seneka snakeroot to be taken frequently; the bark and wine to be continued, and the ulcers dressed as before. Pain of the shin in-

creased, and at one or two points about midway over the tibia pale red spots begin to appear. 16th.—Spots on the shin now about the size of a quarter of a dollar, are thin, and matter forming and inclining to collect on the external side of the leg. We now applied cloths wrung out of a strong decoction of white-oak bark sufficient to cover the whole limb; continued the Peruvian bark internally, and the same local treatment. Aphthæ of the mouth and throat is very acrimonious, and occasions great difficulty in taking either food or drinks; directed an infusion of nux gallæ and alumen in hot water, for a gargle; this was soon of great benefit in clearing away the foul sloughs, but for a short time they formed again, though of a milder nature and less confluent; we now put her upon an alterative course of calomel to excite a gentle ptyalism, and continued to support her with bark and wine, with cordial nourishment; a mild alkaline and aperient solution of phosphate of soda in water was also directed to be taken frequently.

18th, 19th, and 20th.—Symptoms nearly stationary; same treatment continued. Notwithstanding the application of the saturnine solution, a strong decoction of white-oak bark and brandy were assiduously employed on the limb; still the parts were in such a state of debility as to put on very threatening appearances of mortification. Fortunately, however, for the safety of the limb, it terminated in a large abscess, deep seated in the muscles, on the outer part of the leg, occupying a space of about six inches, and in which a fluctuation of matter was to be felt, and which made its way through an opening or sinus formed into the large ulcer on the foot.

By this discharge an important truce was obtained, the patient was more comfortable, and the common integuments acquired a solidity that was perceptible to the touch. A short time after this the surface of the ulcers became so acutely sensible, that it was necessary to substitute a mild cerate for the strong digestive, notwithstanding which our patient soon began to be tortured with the most excruciating pains, sometimes in the toes, sometimes in the heel, and at others in different parts of the leg, up to the ham, and which continuing to return daily, lasted a long time. During

this distressful period, we continued to support her with the bark and other tonic remedies, with wine and cordial nourishment, and we were under the necessity to use opiates freely to mitigate her very extraordinary sufferings. A gentle ptyalism now appears from the previous use of the calomel.

As the discharge from the abscess, through the sinus to the ulcer on the foot, was likely to prove insufficient for a radical cure, a large seton was proposed, and on the 7th February was introduced into the superior part, and extended downwards to the most depending point of the abscess.

This mode of effecting the entire discharge from the abscess was preferred to a large and deep opening with the knife, for obvious reasons, which will be readily understood from the foregoing history of this terrible case. During the greater part of the disorder the left leg was more or less swelled, and was of the same glossy and deadly pale appearance, especially the foot, which at one time became so enlarged and œdematous as to require puncturing, after which it subsided with a gradual discharge of water from the punctured parts. The debility and laxity of the parts of this leg also became so considerable as to require daily frictions with brandy, and the application of a bandage.

On the 10th February, three days after the introduction of the seton, I relinquished my attendance on this case, as I now considered her in a safe situation, though for some time prior to this we had serious apprehensions for the fate of her limb, and even her life.

I now left the patient in charge of my friend Doctor Samuel White, more especially as it then required little else than surgical assistance, calling after this as often as my health at the time would admit of.

The seton having been duly attended to and maintained, until the abscess was perfectly emptied of its contents, it was then withdrawn, and with the steady application of a bandage to the leg, and proper attention to the system, the sides of the abscess at length coalesced, and the ulcers on the foot gradually healed, though many weeks elapsed before the completion of this desirable event; but the patient's general health was in a great measure

restored to her, and she was enabled to attend to her family concerns; yet she remained lame from the destruction of the tendons, as above related, and from the contraction of the limb, and is unable to walk without the aid of *crutches*.

REMARKS.

Medical writers appear to have entertained different opinions relative to the nature and treatment of this disease; and it was not until the year 1784 that Mr. White furnished the first regular treatise on it in Great Britain, and at different periods since that time, it appears that Mr. Trye of Gloucester, and Doctors Ferriar and Hull of Manchester have written on it. Doctor Ferriar is of opinion that phlegmasia dolens puerperarum may exist independently of every circumstance regarding parturition, and he does not think it impossible for it to take place before delivery. Doctor Miller tells us that he had under his care a case of this complaint in an aged woman, and of course unconnected with parturition. Doctor Denman's opinion is, that the absorption of vitiated matter from the uterus is the cause of the swelling and pain of the inguinal glands, and all that follows, but at the same time he does not think that this vitiated fluid is produced by the retention of coagulum or of any portion of the placenta or membranes, but is consequent to an unhealthy state or morbid action of the uterus itself.

Most medical writers I believe maintain that this swelling is not œdematous. Doctor Burns asserts, that "if the skin be punctured, no serum is effused, at least not in the same way as in anasarca, and the swelling is not increased in a depending posture." In the more common or milder forms of the complaint I believe this may be true, as in such I have not observed the characteristic symptoms of an œdema to occur; but the case we have recorded must be admitted as an exception to these general assertions, for we could discover no difference between the local affection in this case and an anasarca, as that disease invariably appears.

On Temulent Diseases. By Joseph Klapp, M. D. one of the Physicians to the Philadelphia Alms-house Infirmary.

"May man never forget that the vine sometimes produces very bitter fruit,—disease, pain, repentance, and death!"

Some late particular experience induces me to solicit the attention of the faculty to the pathology, but more especially to the treatment of that kind of vertigo, mania, and epilepsy, which are produced by the inordinate use of ardent liquors. They are, I believe, equally gastric in their origin, and so similar in nature as will justify a declaration of their being only different members of the same group or family of diseases. That these three complaints are derived from the same or a similar morbid state of the stomach, induced by its having been unduly stimulated with ardent liquors, is a position which I trust will prove to be well founded. 1st. On the sameness of their remote cause; 2d. on their symptoms; 3d. on certain facts; 4th. on dissections; and, 5th. on their method of cure.

A priori, it will be admitted as a reasonable supposition that the diseases in question consist in, or at least are connected with, a depraved state of the digestive organ. The cause operating so directly on that organ, it would seem almost impossible for it sooner or later to escape falling into a morbid condition. The well known dyspeptic complaints, which detract so much from the pleasures of the followers of Bacchus, indicate such a change to have taken place. Hence those giddy votaries are so frequently complaining of a loss of appetite, flatulency, diarrhœa, vomiting in the morning, &c. These symptoms of a vitiated state of the stomach alike attend each of the diseases above mentioned; this fact, joined with the knowledge of their remote cause being the same, is calculated to impress one with a belief of their being of a similar nature.

In vertigo the dyspeptic symptoms more constantly attend. Epileptic patients are, I believe, always more or less harassed

with nausea. In the cases which will be presently related, frequent reachings, or ineffectual vomiting, constituted a prominent and leading symptom. In the first and convalescent stages of drunken insanity, it is well known that patients refer their complaints chiefly to the stomach.

Certain facts have come within my knowledge, which I think will be admitted to afford some support to our gastric pathology. These have a tendency to show that in habits predisposed to the epilepsy and mania under consideration, irritants of different kinds are capable of exciting those diseases. The doctrine also derives additional support from the circumstance of those diseases almost immediately ceasing to exist upon extracting from the stomach their noxious causes. The first of the facts alluded to I will transcribe from a case of mania a temulentia described in my communication on that disease in the 7th vol of the Eclectic Repertory. "Twentieth of August, while the patient was convalescent he requested to have something to eat; I would only permit him to have panado or gruel containing a small quantity of brandy, and he was allowed for drink some weak wine and water. 21st. My directions had been deviated from; the patient was indiscreetly permitted to eat several small birds; shortly afterwards he began to complain of a sense of heaviness, and of pain in the region of the stomach and back; in the night slight but evident aberrations of the mind were again noticed, and his rest had not been so comfortable. Suspecting that the stimulus of his yesterday's dinner had occasioned these symptoms, I desired that the emetic solution should be again given to him.

22d.—The medicine had operated freely after giving the first dose, the uneasy feelings about the stomach from that time ceased, and he slept comfortably through the night.

23d.—The patient to-day was found walking about the house in every respect well, and the attendance was discontinued."

This case exhibits, I think, very clearly a specimen of derangement of mind, symptomatic of a primary disease of the stomach, induced by the irritating effects of aliment which it was incapable of digesting. As soon as the emetic had relieved the pain and op-

presion about the stomach, by the displacement of their cause, the mental disease disappeared.

The second fact I will submit, of an import, and admitting of a construction, similar to the first, I have been supplied with in the case of a patient who had been cured of lunacy from intemperance in the infirmary of the alms-house. While convalescent he was liberated from his cell, and permitted to walk in the yard. Shortly after his enlargement he privately eat a quantity of hard, indigestible bread, and then drank freely of cold water. He soon began to complain of his meal sitting heavy on his stomach, derangement of mind speedily supervened, and it became necessary to remand him to his cell. The cause of his disease having been satisfactorily discovered, an emetic was administered, which operated briskly, and brought off the bread he had eaten, and shortly afterwards he became again rational.

A third patient, with the same disease, while convalescent, was induced, through the persuasion of some anxious, officious friend, to take a raw egg with wine in the morning to strengthen the stomach: the dose proved indigestible; his stomach soon became uneasy, and epileptic convulsions, with derangement of mind, ensued. In the course of a few hours, after many remedies had been ineffectually applied, he took something, at present not recollected what, which acted as an emetic, and the egg was disgorged entire, or nearly as much so as when it was swallowed. After this the epilepsy and mania ceased, nor had he any return of them from that time.

I trust it will be conceded that these facts reflect some probability on the theory of the gastric nature of mania a temulentia, or, as has been before intimated, they prove that, where there exists a predisposition to that disease, other irritants besides spirituous drink are capable of producing the same kind of lunacy, or a disease so nearly resembling it, as not to be distinguished from it by the symptoms. The result of the mode of treatment further evinces that they are equally gastric, and from observation I do believe them to be one, and identically the same complaint.

This doctrine has also derived support from the dissection of the bodies of those who have died with madness from intemper-

ance. We are told that in many cases no mark of disease whatever has been discovered in the brain, when both the liver and stomach have been inflamed.

Finally, that the three temulent diseases enumerated above have a gastric origin and nature, is rendered probable by the safety and facility with which they are removed by the employment of emetics.

If it be admitted that the diseases whose pathology I have been endeavouring to illustrate, consist of a morbid condition of the stomach, the indication of cure from thence to be deduced is rendered very evident. To remove offensive matters which impress a morbid influence on the nerves of that organ, and more especially to alter its state so as to enable it to perform its healthy functions, the use of emetics is clearly indicated. The subjoined cases will exhibit the effects of this practice. I will begin with vertigo.

Mr. ——, about 50 years of age, has long been in the habit of using strong drink to an excess. On the 1st of June last, my assistance in his case was desired, and of which he gave me the following account. For a length of time he had been subject to bilious puking in the mornings, a poor appetite, and vertigo of a severe kind. The last affection often seizes him while walking about, and sometimes induces a degree of debility in his limbs nearly causing him to fall. This morning he suffered a severe attack of this kind, in consequence of which he fell on the floor, though the disease did not extend so far as to occasion either convulsion or a loss of sense at the time. His skin was cool and the pulse nearly natural, beating about 63 times a minute.

He was directed to take two grains of tartris antimonii dissolved in a little warm water, every fifteen minutes, until the stomach was freely evacuated, and while operating to drink warm water copiously. June 2d, on entering his chamber, he accosted me with the following remark: "I am much better to day, and have a strong appetite, which I have not before known for a long time." Eight grains of the tartar emetic had been taken when vomiting ensued, and the stomach was well deterged of its morbid contents. It also purged him twice freely. When the operation was over he

found his vertigo was gone. The pulse this day stood at 84 and somewhat weak, his skin remains cool, and he now complains of nothing but a sense of general weakness. He was ordered a light diet, and the use of bitters.

June 3d. Has had no return of the vertigo, and his appetite is unimpaired. Continue bitters.

June 4th. Remaining well, further attendance was omitted.

Cases showing the efficacy of this practice in mania a temulentia would be superfluous in this place, a sufficiency of which having been already detailed in the 2d No. of the 7th volume of the Eclectic Repertory, to which publication I beg leave to refer for further information. I am, however, at present desirous to show in a different way the great advantage of the practice, by exhibiting a plain and a fair comparison between the old methods of treating the disease, and that which I have ventured to recommend. As well ascertained facts, properly stated, often impress upon the mind more conviction than volumes of disquisition, I shall let the comparison consist of simply transcribing two cases of similar degrees of violence, which are already before the public, the one treated with opiate, &c. and the other with emetics. The first case is recorded in the 14th chapter of that most estimable work, the Medical Reports of the late Dr. Currie, and in the following words.

June 2d, 1796, L. I. aged 32, a man of very irregular habits of life, was admitted into the asylum in a state of furious insanity. His disease was supposed to have been brought on by excessive drinking. As cases of insanity including the whole method of cure are seldom recorded, I will give this in detail.

Very powerful methods of coercion were required in the first instance, and the bowels were afterwards cleansed by a saline purgative. At night he took eighty drops of laudanum, which produced imperfect sleep, and in the morning following he was more wild than ever.

Having in some cases of insanity found very extraordinary effects from such doses of opium as induced profound sleep, I desired that five grains of this medicine should be given every four hours, till sleep succeeded. Next day (4th of June) it was re-

ported to me, that he had taken twenty-seven grains of opium in twenty-four hours. From the first five grains there was no apparent effect; after the second he slept an hour and a half; after the third, three hours. Though the medicine had been since twice repeated, in doses of six grains each, he had no return of sleep. He appeared wild, agitated, and feeble, rather in a state of alarm than of fury. His pulse 100 and soft, with gentle and general diaphoresis.

It seemed not prudent to push the medicine farther; but having often in cases of this nature observed the soporific effects of opium to come on many hours after the medicine had been discontinued, it was directed that he should have milk as his food, and that medicine of every kind should be omitted for twenty-four hours.

The report of the fifth is as follows. " He remained in a perpetual state of alarm till eight o'clock last night; after that hour he slept, but not soundly. He has taken nine pints of milk in all, having been very thirsty. It agreed with him till ten this forenoon, *when he vomited considerably. Since that time he has been more tranquil and feeble.*" In this state, I continued the directions to omit all medicine, ordering him a pint of wine, with two pints of gruel, daily, and such solid food as his stomach could receive and digest. This plan was followed till the 17th, when his strength was considerably restored, but his insanity as violent as ever. It was now directed that he should go daily into the bath, heated to 95 degrees, and that he should take half an ounce of the infusion of the digitalis thrice a-day, as recommended by my friend, and kinsman, Dr. Currie, of Chester. (Mem. of Med. Society of London, vol. iv.)

He continued on this plan with little variation, till the 29th; the report of that day is as follows: " Till the 25th the digitalis produced no obvious effect. On that day sickness came on, and his pulse was hardly perceptible. The digitalis was therefore omitted. He has been regularly in the bath, and while in it was once seized with *deliquium animi*, but the mental derangement did not afterwards abate. His pulse is now about 94, feeble, and irregular. He is a good deal emaciated, but more fierce than ever; he

talks incessantly, with the utmost incoherence, and the association of his ideas seems wholly perverted."

In this state I ordered the digitalis, and the tepid bath to be discontinued, as well as all other medicines. I directed a nutritious diet, but without wine or opium, and placed him in a situation to be as little as possible exposed to external impressions. For three days there was little change in his situation—his appetite improved, and he sweated much. At the end of this period, he had suddenly a lucid interval of several hours duration; but before I could witness it, he was again become furious.

At this time, (the 16th of July,) I directed that he should take three drachms of Peruvian bark, half a drachm of vitriolated iron, and the same quantity of aromatic powder, in the form of an electuary, daily; and that twice every day he should be subjected to the tepid affusion for a minute or two at a time, the water being poured gently over him of the heat of 88 degrees.

The following is the report of the 16th of July: "The tepid affusion was continued till the 14th, but without any tranquillising effect; the other medicines have been given regularly. On the 9th he became calm, and rational, and slept the following night for eleven hours profoundly; but started suddenly from this sleep, as wild and as furious as ever. This state of mind continued twenty-four hours, when he again became tranquil, and continued so till he went to bed. He slept a few hours quietly, but awoke as before, under extreme agitation, and continued fierce and raving for twelve hours and upwards. Since this time he has had a succession of paroxysms of insanity, alternating with periods of tranquillity of mind, of nearly the same duration. In general he goes to bed tranquil, and sleeps quietly, till about three in the morning, when he awakes under the horror of some frightful dream; and this introduces his paroxysm of furious insanity, which continues from ten to fourteen hours."

In this situation I ordered an emetic to be administered to him late in the evening, in hopes that it might prevent the return of the paroxysm. Being disappointed, I had again recourse to an opiate at bed time; and this also proving inefficacious, I directed that he should be awoke before the expected accession, and kept

awake beyond its period of attack. By these means we disturbed the regularity of the alternations, but without any advantage; on the contrary, his lucid intervals became shorter in duration, and less complete.

Perplexed with these extremes, and having in mind the success of the cold bath in convulsive diseases, I ordered it to be tried on the present occasion. The insanity returning with great violence, on the 21st he was thrown headlong into the cold bath. He came out calm, and nearly rational, and this interval of reason continued for twenty-four hours. The same practice was directed to be repeated, as often as the state of insanity recurred.

The following is the report of the 30th: "The direction has been followed, and on the morning on the 23d, he was again thrown into the cold bath in the height of his fury, as before. As he came out he was thrown in again, and this was repeated five different times, till he could not leave the bath without assistance. He became perfectly calm and rational in the bath, and has remained so ever since."

"This patient continued with us for some time afterwards, bathing every other day, and taking the oxyd of zinc in small quantities. He never relapsed, and was discharged some time afterwards, in perfect health of body and mind."

The case which I would wish to contrast with the one just related will be met with in the 2d No. of the 7th volume of the Medical Repertory before referred to. In this it will be perceived that reason was perfectly reinstated in three days, instead of the protracted, tedious period of seven weeks, the time before Dr. Currie's patient was cured. This case has been selected out of several others there submitted, for two reasons, 1st, because it was a case of great violence, at least equal in this respect to Dr. Currie's; and 2dly, because it was treated in the lunatic department of the alms-house in the presence of the students then attending the practice.

"On the evening of the 2d of October, 1814, G—— was admitted a patient into the alms-house, for mental derangement of the furibund grade, and satisfactorily ascertained to have been occasioned by the excessive use of ardent liqours. He is a carpenter

by occupation, is about 40 years of age, and on inquiry I was told that for many years he had been a very intemperate man. Mr. Thum, the senior house pupil, assured me that at the time of his admission, he was not only violently insane, but he appeared to be so on every subject. To prevent injury to himself, it was found necessary, in addition to the use of the strait waistcoat, to have him closely chained to the floor of his cell. His pulse was frequent and weak, and the trembling of his limbs was very great. He took two grains of the tartris antimonii dissolved in a little warm water every fifteen minutes; vomiting was not created until after twenty grains had been administered, when a large quantity of viscid mucus, and sordes were brought up, and afterwards the bowels were smartly purged. After the operation of the antimony was over, Mr. Thum found that the pulse had become less frequent, and fuller. The patient had some sleep during the latter part of the night.

3d.—He seems much better, pulse remains as last stated, he speaks correctly on some subjects, but remains evidently deranged on some others. On entering his cell we found him very curiously employed: he was pressing with both arms, and apparently with all his strength, against the wall. I requested an explanation of his conduct, but he refused to give me his reason for doing so. The cell keeper afterwards informed me, that during the whole morning, he had been bearing against the wall, through a fear, as G— had stated to him, that unless it was held up, it would certainly fall upon him and crush him to pieces. I desired the tartarised antimony to be repeated.

4th.—When sixteen grains had been given, first vomiting and then copious purging ensued. On examination the pulse proved to be 100, and weak; he is plainly more rational in both conversation and behaviour; in short it was rather difficult to discover that there was any of the mental affection remaining. In the course, however, of my conversation with him, it was perceived that his intellectual faculties had not yet been perfectly reinstated. Prescribed blister plasters to the ankles.

5th.—Out of the last twenty-four hours he has slept eighteen, and is still inclined to sleep. When questioned about sleeping

so much, seems conscious of his indolence, and apologised for it, by saying that the narrow limits of his cell would not admit him to take exercise, so he thought he might as well sleep as not. The blisters had not yet been dressed. By the test of conversation he this day proved rational upon every subject. The tremor of the limbs have totally ceased, pulse remains rather too frequent, but has more strength than at any time heretofore. Nothing prescribed but some light diet.

6th.—As yesterday. On entering the cell I found G— was still very comfortably reposing in bed; he stated to me that his sleep had been uninterrupted through the night, and asserted that, with the exception of a little weakness, he now felt as well as he had ever done. The debility of which he complained, he jestingly attributed to the fatigue he had incurred by the great exertions which he had made in holding up the walls of his cell. I embraced this favourable opportunity of asking him why he had put himself to that unnecessary trouble, he replied that he had done so out of a flighty impression of his being at that time employed in erecting the partition of a large house, which required holding to prevent it from falling; at which deranged belief he now seemed much diverted. The pulse was 100, and had acquired fulness. He speaks sensibly on every subject, and gave me the following very amusing history of the beginning, and advancement of his insanity. He said it had been coming on better than a month, as far as his recollection would serve him; his first deranged impression occurred to him at night, while in bed with a fellow workman. Two men, as he supposed, had furnished themselves with long iron hooks, with which they pulled off him his bed covering, but he grasped them with his hands, and again covered himself; after which they were several times pulled off the bed by them: this irritated him; and in a violent passion he jumped up to take revenge on these audacious fellows who had so repeatedly attempted to deprive him of his rest. At first he endeavoured to get hold of these persons, but they had the address and activity to elude his grasp; though while pursuing them around the room, he was sometimes on the very point of securing them. In regard to the actual presence of these mischief workers, there seemed to him to be no

deception; for he plainly saw them skipping, and running in different directions to avoid him, and he was only surprised at their possessing so much more activity than himself. At one time he was almost certain of having one of them, as he came so near to him as to catch hold of his long iron hook, the instrument with which he had been disturbed while in bed. At length being much wearied by this unsuccessful chase, as a last effort to be revenged, he stooped down, and picked up a stone, which he sent at them with great force; but they again contrived to escape injury by making a sudden spring around the corner of the room. He now retired to his bed, but in a short time afterwards, his attention was a second time powerfully excited by a full band of music, which had suddenly commenced playing in the street immediately before the door; he arose, and on opening the window to look out, he was unexpectedly surprised by the sight of a most splendid edifice, which had just been erected in the street; the building possessed many ornaments; among the rest it had an elegant dome, which was covered with stars of the most brilliant kind. After gazing with indescribable admiration for some time at this grand fabric, he perceived two persons having hold of hands, dancing round each other in the cupola, instantly he recognized them to be the two men above alluded to; they called to him, and invited him to come, and join them, which he did; and after dancing with them a short time, they asked him to accompany them to the theatre: with this request he also complied. The farce which was to be acted was called the Broken Bridge; they wished him to perform the part of the carpenter, to repair the bridge; but this he refused to do, which so much affronted them as to induce them to attack him, and beat him very severely. These strange adventures G— states were acted over two or three successive nights, when he became so unmanageable as to make it necessary for his friends to have him placed in the lunatic department of the alms-house. Of the inconsistency of his past opinions and conduct, stated in his curious recital, no one could be more sensible than himself, or more diverted with its folly. Being in every respect well, the patient requested his discharge, but was prevailed on to delay his departure a day or two

longer. The pupil of the house was directed to furnish him with a little spirit and water for drink, and to give him panado containing some spirits, for sustenance.

7th.—Continuing to be well, I consented to his liberation."

In page 261 of the 2d No. of the 7th volume of the Medical Repertory, I have suggested that the use of the emetic in mania a temulentia complicated with epilepsy, would be unsafe. I have, however, since become convinced, from more considerable experience, that this apprehension was entertained without just foundation. The passage in which that opinion was intimated is the following: "Having formed an opinion unfavourable to the use of emetics when this disease (mania a temulentia) is accompanied with epileptic fits, I feared it would be improper to administer the antimony in this case." The case alluded to was madness from strong drink, complicated with some kind of convulsions, the real nature of which I was not able to ascertain, from the very imperfect information obtained from the friends of the patient. My fears of the impropriety of emetics in such complications of mania, in justice to myself, I must state, had been created by some information communicated to me of the experience of others, and not by any thing which I had witnessed in my own practice. In this I conclude there must have been some misapprehension, for I am now confident that the practitioner may not only fearlessly, but successfully administer the emetic in a great proportion of such cases. From numerous trials since the date of the publication above referred to, I have ascertained satisfactorily, that so far from the accompaniment of epilepsy forming an objection to the use of the emetic, that that disease, when the effect of hard drinking, whether it occurs in conjunction with mania, or exists in a separate state, is itself safely and promptly removed by it. The following cases have been selected as specimens of this practice.

T. F. aged 47 years, for a length of time in the habit of drinking to an excess, during the last week or ten days has been affected with sleepiness, a loss of appetite, pain in the breast, and tremor in the limbs.

On the 21st of May of the present year, at 10 o'clock in the

morning, he was suddenly seized with a violent epileptic fit, which continued fifteen minutes.

On the 22d he fell into a state of mental derangement, and,

On the 23d, I found him labouring under several diseased perceptions, particularly from sight and hearing. His false perceptions and insane thoughts are of this kind. He is constantly engaged in picking up in different parts of the room certain imaginary objects, and seems desirous of having them all together in one place. He charges his wife with having conspired with a number of persons for the purpose of killing him, and to carry off his property. He frequently declares that during the whole of last night his house, in consequence of the efforts of these conspirators to plunder him of his property, and to carry him off, was made a constant scene of bustle and contention. His skin is covered with a cool, or rather a cold perspiration, the tongue is partially covered with a brown fur. The pupils of his eyes are a little dilated, has no pain in the head. Through a fear that I was actuated by some improper motive towards him, he would not permit me to have his hand long enough to feel his pulse. I ordered two grains of tartarised antimony to be given to him dissolved in water, every fifteen minutes, till vomiting was freely excited. After which to have him kept as quiet as possible to encourage sleep.

24th.—After three doses of the antimony had been given, he vomited considerably, and discharged a large quantity of a dark-green coloured sordes. When the operation was over, of his own accord he went up into his chamber, laid down, and slept a little. When he awoke his insanity continued, but to a less degree than before; he was very restless through the night, and this morning before I saw him his wife again gave him the emetic, which operated both upwards and downwards, severely. Neither yesterday nor to-day, while the emetic was operating, could any disposition be perceived to a return of his epilepsy. Mrs. D. this day assures me that each time after using the emetic she could perceive an evident abatement of his mental derangement. For the first time, to-day he would permit me to examine his pulse; it was 134 a minute, and possessed some force. His skin remains moist, and has acquired its natural temperature. The tongue has

become clean and moist. A blister plaster was directed to be applied to the back of his neck. A light diet was enjoined, and the use of spirituous drink forbidden.

25th.—When I entered his room to-day, he said a great change had occurred in his case since yesterday, a remark with which I was pleased, as it indicated a return of reason. I asked him if he recollected what was the matter with him yesterday, his reply was that he believed he had been out of his senses. He complained of the blister being very sore, and by other observations satisfied me at once that he was speedily recovering. Since my visit to him of yesterday his wife informed me that he has been almost constantly in bed, and has slept the greater part of the time. For a short time to-day he was again deranged, but afterwards he went into a sound sleep. When he awoke he was sensible, and has since remained so. The pulse is now only 100, and the tongue and skin natural. A continuation of rest and the light diet were directed.

26th.—In mind he now appears to be quite well, but complains much of weakness and a soreness in all his limbs. I prescribed half a cup full of bitter tea to be taken several times through the day. The tea was made by infusing bark, serpentaria and chamomile, of each two drachms, in a quart of boiling water.

27th.—To all appearance being at this visit well in both body and mind, I concluded to take my leave of Mr. D.

The two following cases were well marked, and exclusively epileptic. Though the subjects of them have several different times been affected with paroxysms of the disease of the severest kind, yet neither of them has ever been known to be maniacal.

Mr. L———, aged 40 years, an intemperate man, during the last five years has been subject to attacks of epileptic convulsions. His wife informed me that for some time previous to my seeing him, he had been affected with vomiting in the morning, pain in the head, a disposition to sleep often through the day, and that he had been disinclined to activity of every kind. During the night of the 27th of May, 1818, the pain in the head was constant, and unusually severe. Much sickness at the stomach attended, and he slept scarcely none. Between eight and nine o'olock of the

morning of the 28th, he was suddenly seized with an epileptic fit. I was told that while in that fit, he was entirely insensible, his countenance was very dark, a frothy saliva issued from his mouth, and his whole body was convulsed. In this state he continued full half an hour, gradually the paroxysm went off, and after a while he came out of it entirely. He still continued to complain of head-ach, and sickness at the stomach. Between eleven and twelve o'clock of the same day he experienced another severe fit, in which his breathing was stertorous. This continued not quite half an hour; shortly after he had recovered from this I paid him my first visit, and desired him to be cupped on the forehead and temples, and strong sinapisms to be applied to his feet and wrists. At four o'clock P. M. notwithstanding the remedies had been promptly used as I had directed, he suffered a third attack, which for duration and violence was fully equal to those before mentioned. My second visit was paid at eight o'clock in the evening; at that time he was sleepy, easily roused, and rational. Every now and then he would become very sick at the stomach, frequently making efforts to vomit, and at the same time complaining of violent pain in the head. The pulse is firm, and 72 a minute, the tongue is moist, skin warm, and his countenance flushed. Two grains of tartarized antimony dissolved in a table spoonful of warm water were directed to be given every fifteen minutes until free vomiting was excited.

29th.—Two doses operated smartly, by which his stomach was released from three pints of a green liquid, which he said was extremely bitter; a quantity of a tough, brownish mucus, was also brought up. I was with him while under the operation of the emetic, and, though he vomited with much violence, not the slightest tendency to a return of the epilepsy could be perceived, even the pain in his head directly after he was done puking was not increased. From the almost constant nausea, and reaching of yesterday, his stomach to-day has passed into a composed state; and instead of having no sleep, as was the case night before last, last evening, when the antimony was done operating as an emetic, he went to sleep, and rested naturally till morning. The medicine also operated as a purge, and produced two large bilious

stools. He still complains of head-ach. The pulse is 64, and natural as to size and strength. The skin is embued with a gentle perspiration, and is moist, with a slight covering of whitish fur in the centre. With the view to lessen the pain in the head, pediluvium and mustard poultices to his feet were ordered. He was directed to drink cold chamomile tea, and to use a light diet.

30th.—His rest last night was uninterrupted, and his sick stomach has been exchanged for a good appetite. The vertigo and pain in the head have entirely ceased. The state of the pulse and tongue accords with other favourable appearances. He was permitted to leave his chamber and to walk about the house, but not to go out. The bowels being somewhat costive, he was ordered a purge of calomel and senna infusion.

31st.—The medicine produced several dark-coloured stools. The bitter tea to be resumed.

June 2d. I met him to day in the street, when he appeared to be in good health.

Mrs. S———, aged 43 years, addicted to the excessive use of ardent liquors, has been subject to epileptic fits, to vomiting mornings, imperfect appetite, and other symptoms of a dyspeptic nature for four years. Of the present attack I received from the family the following account. On the 2d of August, 1818, she complained of pain in the stomach, the next morning she had a severe spell of vomiting, and had considerable vertigo; about the middle of this day she fell into a violent epileptic fit, which continued about half an hour; during the remainder of the day she had two additional ones. On the 4th of August, she had three epileptic fits more. Immediately after the fit terminates she regains her reasoning, and says that the paroxysm always comes on with these feelings: first she is unusually sick at the stomach, then her head becomes giddy, and the fit follows directly afterwards. The pulse is 92, and has some force; the tongue is natural. Previous to my visiting her she had been bled, her feet had been bathed in warm water, and mustard plasters applied to them. She had used æther and laudanum freely. I directed

two grains of tartar emetic to be given every fifteen minutes until a sufficiency was taken to operate freely.

August 5th.—After three doses of the antimony had been taken, puking was excited, and a large quantity of biliary sordes were rejected; purging succeeded, and many dark-coloured stools were produced. When the medicine was done operating, the patient became very sleepy, and obtained, for the first time since the disease began, some comfortable and natural rest. This morning she is free from giddiness in the head, has been able to walk a few times across the room, and her health appears to be every way improved. Those who were with her while the vomit was operating, inform me that they did not remark any tendency to a return of the epilepsy. The pulse is now 79, and natural as to volume and regularity.

6th.—About one o'clock yesterday she experienced a return of vertigo, and feared another attack of her disease, but this did not take place; in a short time the giddiness went off. Last night she rested well, and to day she is about her chamber.

8th.—At my visit of this day she appeared well in every respect, and the attendance was omitted.

Sept. 1st, 1818.

On Mania a Potu. By Joseph G. Nancrede, M. D.

Among the numerous evils with which humanity is afflicted, none are more calculated to attract our attention, or to excite our sympathy, than those which have their seat in the intellectual faculties. Corporeal sufferings will ever inspire the heart of the humane and benevolent with feelings of interest and compassion, and their removal will be attempted with zeal and perseverance; but the interest this class of diseases is calculated to excite, bears no comparison to the heart-rending torments we experience on seeing our relations and friends degraded from the condition of man, the victims of disordered intellects.

Maniacal affections have been divided by authors into several distinct species, according to the apparent causes which have produced them. Each form of this afflicting disease claims its due proportion of attention from every true cultivator of the medical profession: but out of the various species of intellectual derangement, there is one which appears peculiarly entitled to the observation of the American physician. Mania caused by intemperance is a form of disease too frequently observed in our country. Causes, the investigation of which would be entirely foreign to the object of these remarks, have tended to diffuse throughout the United States a greater use, and consequently a greater abuse of spirituous liquors, than it is supposed could be found in any other country whatever. Mania from intemperance, and its numerous cases, are therefore readily accounted for. On the other hand, by consulting the European writers, who have treated of intellectual derangements, we find that this cause gives rise to a very small proportion of the maniacal cases they have recorded. It appears to have been reserved for our late lamented Rush, who, during a long career of useful practice, had so frequently witnessed cases of this disease, to be the first to call the attention of medical men to the importance of this cause. His *Medical Observations and Inquiries on the Diseases of the Mind*, inform us, that, at one period, the influence of this cause was so great, that it produced one-third of the whole number of maniacal patients in the Pennsylvania hospital. This fact is of sufficient importance in itself to point out the necessity of turning our attention to a disease which it becomes us to notice more particularly, because it prevails more frequently in America than in other countries; and because our mode of treatment must be founded upon our own observation. The frequent recurrence of the disease in private practice, and its alarming increase in our public institutions, are deemed considerations sufficiently strong to authorise the publication of the following case.

Francis A——, aged about 40 years, of a middle stature, rather thin, florid complexion, and of a constitution naturally strong, a weaver by trade, residing in the western part of the city, came under my observation in June, 1818.

In the latter part of the late war, Mr. A. then residing in Germantown, had met with some considerable losses in his business, which, with some other unpleasant occurrences, induced him to remove to Philadelphia, where I have had many opportunities of seeing and conversing with him while in attendance upon sick members of his numerous family. His appearance and conduct at all these times were very far from those of a man in any ways deranged. However, the depression of spirits under which he laboured appears to have induced him, even at this early period, to resort to the occasional stimulus of wine and spirituous liquors, which soon brought on paroxsyms of intoxication, during which the patient, as his wife informs me, was usually furious and abusive. These attacks generally passed away as soon as he had obtained sleep. It does not appear that his bodily health suffered materially from this mode of life for several months, and although he indulged occasionally, but at very long intervals, in this vicious habit, he was still attentive to his work and industrious. But about the month of August, 1817, he gradually lessened the intervals of sobriety, until, at last, almost every week brought with it a new paroxysm. He remained in this situation until the month of June, 1818, when business induced him to visit Baltimore; from whence he returned afflicted with the disease now under consideration.

For an account of his proceedings since he left Philadelphia, as well as for the apparent cause of his disease, we are obliged, for want of other information, to rely upon his own narration, made since his recovery. It would appear, that on leaving Philadelphia, and while on board the steam-boat, he became suddenly impressed with fear, and with the idea that his fellow-travellers were conspiring to take his life: for this impression he can assign no cause. In this state of uneasiness he arrived in Baltimore, and took up his lodgings in a tavern, which that night was the abode of several members of a particular society. These men conceived that Mr. A. was also one of their members, without his having given them the least ground for such belief; but finding out their mistake, they charged him with the intention of deceiving them, and threatened vengeance. This occurred in the course of the

evening while at supper. Mr. A. afterwards retired to rest, full of the idea that they would carry their threats into execution. While in bed he was suddenly roused from his sleep by the entrance of his landlord into his room. Mr. A. became immediately possessed with the fear that he was an assassin come to murder him; and so great were his apprehensions, that he attempted, in his desire to escape, to precipitate himself through the window of a third story, and would have executed his project had it not been for the timely interference of his landlord. Persuasions were used in vain, to convince him that he had nothing to fear; he would listen to no remonstrance, but left the house in the midst of the night in search of another lodging. Here he attempted to sleep, but could not succeed; visions were constantly floating before his diseased imagination, representing death, under every shape, with complicated horrors. The succeeding days of his residence in Baltimore presented the same state of bodily and mental disease, without any sleep or alleviation whatever. Having remained there about eight days, he now returned home without having in any way attended to the object of his journey.

He arrived in Philadelphia in the afternoon of Thursday, June 3, and the ensuing Saturday in the morning I was called in. Upon questioning his wife, I learnt that he had passed two sleepless nights, and had eaten nothing since his return home; and that the only idea which occupied him exclusively was the constant apprehension of losing his life. The face was very much flushed, and occasionally changing colour, the eyes fixed, and the pupil dilated. His features had undergone considerable alteration, his look was haggard, and accompanied with a degree of taciturnity, which could not be overcome. If he became engaged in a reply to a question, his eyes were every moment directed towards the door, with the expectation of seeing assassins enter. Finding his pulse strong and labouring, and his tongue furred, I directed fourteen ounces of blood to be taken from the arm, and the administration of a saline cathartic; the patient having had no passage for 24 hours. His wife was also advised to keep him in a dark room, and as remote from noise as possible.

On seeing him the next morning, June 6th, Mr. A. appeared to

me somewhat relieved. The blood that had been taken presented nothing uncommon, and the bowels had been well opened. Although he had not enjoyed any sleep whatever, having, as his wife assured me, undressed and dressed himself, as much as thirty times in the course of the night, still his features were improved. As soon as he had laid down in bed for a few minutes, he would suddenly rise in great agitation, and assure those who were around him, that he heard a noise, &c. The pulse was regular though still hard, and his conversation was now somewhat more intelligible. The face had assumed nearly its natural colour, though his looks continued haggard; the least noise startled him. His impressions of danger were not quite so constant as the day previous; but when any circumstance recalled them, they were as violent as before.

June 7th.—Being excessively engaged the next day principally by an increase of dispensary patients, I could not find leisure enough to give this case sufficient consideration, in order to determine upon a mode of treatment; and in this dilemma, attending merely to the want of sleep, directed the exhibition of a powerful opiate. More restlessness being observed, his wife was advised to keep him as quiet as possible, and not to lose sight of him.

June 8th.—On visiting the patient early this morning, I was informed that he had passed a sleepless and very agitated night; a continual restlessness appeared to supersede every other feeling. Although complaining of much fatigue, he could not remain in the same position for any length of time. The intellects were more visibly affected this morning than they had yet been; his fears recurred almost every moment, and the impression of noises about the house was constant. It had been found impossible to engage him for three or four days in any lengthened conversation. He now became extremely loquacious, relating a variety of events which had occurred to him, with loud gesticulations; and complaining most violently of the treachery of his wife (all which circumstances have no foundation.) I was now informed, upon minute inquiry into his habits, of his indulgence in the use of spirituous liquors for a long time. But that for some weeks previous to this attack he had not been known to be

intoxicated. In the afternoon I again saw him, and learnt that the patient had been prevailed upon before I had seen him in the morning, to take a small quantity of gin, which might account for the increased loquacity, &c.

June 9th.—Deeming this a proper case for the application of Dr. Klapp's mode of cure, after having mentioned the case to one of my medical neighbours, I determined to exhibit the antimonial wine the next morning, and to have it succeeded by the use of an opiate. The patient objected in a most obstinate manner, but consented at last, after repeated efforts to convince him of the necessity of the measure. On calling in the afternoon, I was informed that the antimonial wine had been administered, and had brought away a quantity of thick viscous matter of a dark or brownish colour. There was evidently some slight amelioration in his feelings, and his motions were also more calm. He was unwilling to take the opium, because he conceived that his wife had mixed poison with it: he obtained no rest whatever that night, —his appetite had entirely forsaken him.

On calling the next morning, June 10th, I found the patient in a state of continual agitation, morose, and unwilling to converse. He agreed, however, to take the antimonial wine, and to have it followed by the opium. A copious evacuation was obtained from the stomach of a bilious nature, and for some hours after there appeared to be some relief;—yet, the laudanum taken in successive doses, until 160 drops had been swallowed, produced no sleep whatever—merely a little drowsiness. The appetite was in the same situation, and the restlessness in the afternoon nearly as great as the day previous.

11th.—The same plan was pursued the day succeeding, with no more success as to the production of sleep; but his appetite was rather better, and more calm prevailed. The fears and apprehensions continued the same, but were now almost exclusively confined to the night.

12th and 13th.—The patient was prevailed upon to allow one day's interval, and to repeat the operation of the emetic the next, which was strictly attended to, without any visible change.

The emetics had relieved him to a certain degree, but the dis-

ease was still very powerfully imprest upon the system, and a new method was thought adviseable, in order to produce a more powerful impression.

14th.—On the succeeding morning the patient consented to use the cold shower bath, which was easily prepared in his own kitchen. Its effect was very visible. His agitation as well as fears were sensibly diminished, though the appetite and sleep had not been ameliorated. The return of night brought with it a renewal of his imaginary terrors, especially at the moment of going to bed.

15th.—The next morning I directed the repetition of the shower bath, and the exhibition of two drops of laudanum. In the evening find the patient better,—he has had some sleep for the first time; it was short and uneasy, and frequently interrupted by his waking up, under the apprehension of immediate death.

16th.—The same course was pursued the next morning, and with increased benefit to the patient; he had more appetite, and more sleep than since the beginning of the attack.

21st.—The shower baths, with rest and solitude in a dark room, and abstraction from all attention to business, were persevered in for five succeeding days, at the end of which the patient was perfectly restored to the use of his reason, and became anxious to return to his work. He did resume it, and has continued well ever since, a period of two months.

The preceding is one of three cases of mental derangement from the abuse of ardent spirits, which have been under my care within a very short space of time. In the first, wherein the intellectual functions had undergone but a partial derangement, and which was of short standing, the exhibition of one dose of antimonial wine was powerful enough to remove, in a couple of days, every trace of the disease. The second case, which is of much older date, and has attained a much greater degree of violence, has resisted until now every attempt to overcome the disease. Spontaneous and artificial vomitings, occasionally relieve its intensity, but the total impossibility which exists of preventing the patient from having recourse to the original cause, renders every prospect of recovery entirely hopeless.

On reading attentively the preceding relation of the third case,

it will be found, that the patient experienced no relief whatever from the use of opium, administered in strong doses; but that material benefit was derived from the operation of the emetic. What has been the agency of the shower bath at that period of the disease, in effecting the cure, remains to be determined.

September, 1818.

REMARKS.

With sentiments of respect for the author of this essay, we will take the liberty of suggesting, that, from an attentive consideration of the history of the case given by him, we are led to suspect, that the disease with which his patient was affected may have, in part, been caused by reverses and disappointments in business. To this opinion we are the more especially inclined, as it appears that those causes did so far influence the mind of the patient, as to induce depression of spirits, and to impel him to the intemperate use of ardent liquor. If our conjecture be correctly entertained, a reason from thence may be derived, why the emetic did not prove as serviceable as was calculated on. So far as our memory serves us, at the present time, the employment of *opium*, after using the emetic, does not form a part of the practice of Dr. Klapp, to which Dr. Nancrede alludes. For particular information relative to the new method of treating mania a temulentia, the reader is referred to one of the numbers of the 7th volume of the Eclectic Repertory, published in this city. Ed.

An Essay on the Effects of Temperature on the Human System.
Communicated by Franklin Bache, M. D. of Philadelphia.

All agents which exert an operation on the human system have been arranged under the two great classes of stimulants and sedatives; there is reason, however, to believe that this arrangement

is the result rather of a partial and superficial view of the subject, than one resting upon strict and logical investigation. In the mode of operation of positive agents, there is indeed a difference which may constitute the basis of a very natural division of them; but this division throws no one of them into the class of sedatives, in the abstract meaning of that term.

This difference in the manner of operation of positive agents, just alluded to, consists in the various degrees of velocity with which they produce their effects; when their operation is comparatively quick, they give rise to what is called excitement, and are stimulants in the common acceptation of the term; when it is comparatively slow, they likewise give rise to excitement, produced however in so slow and imperceptible manner as not to be obvious, but to be inferred by a concomitant expenditure of the excitability: when a stimulant has an operation of the latter kind, it is in common language denominated a sedative.

The term sedative is here used to express the action of a stimulant slowly operating. When applied to digitalis, it has this sense; but it has another acceptation, when used to signify the negative operation of privative qualities; thus it is said that darkness and cold are sedatives.

In this last sense its acceptation is exclusively relative; it has relation to a state of greater stimulation; and in that view it implies a diminution of excitement. This will appear evident, when it is considered, that we are acquainted with no absolute privative qualities; for small degrees of heat or light are called sedative, only because their operation is privative, in relation to that of the usual and grateful quantities of these fluids.

If these views be correct, it would appear that although the use of the term sedative in this last sense may be convenient on many occasions, yet, in a strict scientific investigation, it should be discarded; it can be to no purpose to say that the diminution of any stimulant, acting on the human system, has the effect of lessening excitement, because that such must be the effect is evident; and merely because language has supplied names to express certain relative privations, there is no shadow of reason why their nega-

tive effects should be embodied in a scientific arrangement, and suffered to assume the character of positive operations.

Thus much was thought necessary to be said as introductory to the consideration of the effects of heat, whether in excess or defect, upon the human body; for the principal point of controversy relative to heat, being on the manner of its operation during abstraction, it was deemed proper, before discussing it, to give an outline of the doctrine of stimulants and sedatives, which would be assumed in the reasonings of this paper.

The matter of heat is undoubtedly stimulant; any accession of it to the human system shows this effect. The abstraction of the same fluid produces effects also: as the result of the operation of which agency shall we consider them; whether of the stimulant or sedative?

The opinion is held by many physicians, that the abstraction of heat, in as much as it produces very obvious effects, must be stimulant; and by some these effects are supposed to indicate a positive nature in cold, and to give probability to the opinion of the existence of frigorific particles. On the contrary, others denied that any stimulant effect arose from cold.

It must be acknowledged that a good deal may be said in favour of each of these opinions: for upon an abstract logical consideration of the question, the mind is certainly compelled to conclude that the abstraction of heat could not be stimulant; and yet the arguments used to prove that it is, have the appearance of great plausibility.

It will appear, however, upon reflection, that this difference of opinion among physicians has arisen from their omitting to analyse the phenomena which occur upon the abstraction of heat from the human system. This neglect has prevented them from seeing the subject in that two-fold point of view which is essential to its proper comprehension: for with a view to its effects upon the human system, the matter of heat may be considered, 1st, With reference to its relative excess or defect; and, 2dly, With reference to its velocity during accession or abstraction: considered with reference to its excess or defect merely, it is either stimulant or sedative; but with reference to its velocity, always stimulant.

The human system in the healthy state is supplied from an internal source with a quantity of the caloric of temperature, superabundant when compared with that which is usual in the atmosphere; it is, therefore, in all common cases, undergoing, like heated bodies, the process of refrigeration. At a temperate heat of the atmosphere, one at which the refrigeration of the human body goes on neither so fast as to decrease its usual quantity of floating heat of temperature, nor so slow as to allow it to accumulate, no uneasy feeling is produced; for here the velocity of the caloric in passing off is such as to stimulate irritability, but not sensibility, in consequence of the influence of habit; and the quantity of floating caloric of temperature being the same at every moment, it affords a constant and equable stimulus.

If the heat of the external air be a good deal less than temperate, the refrigeration of the body goes on faster than in ordinary times; consequently, the caloric in leaving the surface has a greater velocity than usual; and, by impinging upon the solids and fluids in passing out, stimulates both sensibility and irritability; and its impression upon the former produces the sensation of cold. As soon as the heat of the exterior of the body is diminished to such a degree, as that, by diminishing the rate of refrigeration, the loss of heat is not greater than the supply, in equal times, the sensation of cold ceases; since the cause of the sensation, namely, the increased velocity of the caloric in leaving the body, is removed. Hence it is that a person, upon going out of a warm room into the cold air in winter, feels at first the sensation of cold upon his face; but after some time, that part becomes of a natural temperature to his sensations. This does not prove, however, that it is really of the ordinary animal heat; for when touched by the hand of a person who has been in a warm room, it will give indications of a minus state as to temperature.

By the same mode of investigation, the effects of heat upon the body, when the temperature of the surrounding air is suddenly increased above temperate, may be traced in all their stages. Here then the rate of refrigeration is first lessened; the heat of the body is next accumulated; whereupon the rate of refrigeration is proportionably increased; until at length, by the recurrence of the

original difference in temperature between the air and the body, the former rate of refrigeration is again established, and further accumulation prevented. The body now possesses an accession of caloric; yet, after the usual rate of refrigeration is restored, there is reason to believe that the sensation of heat is considerably diminished.

In thus tracing the phenomena which attend the process of refrigeration in the human body, it is not meant to deny to it the power of regulating the production of its heat, according to the state of its wants; on the contrary, it is believed that the human system has this power, but it is presumed that it cannot be exerted fully on a sudden exigency.

The subject may be further illustrated by the following examples.

If the hand be placed in hot water, it is stimulated, as well because it receives an accession of heat, as that it receives that fluid moving with considerable velocity; if it be placed in water of a medium temperature, which is gradually increased, it is stimulated by the accession of heat only; its velocity during accession not having any appreciable effect. On the contrary, if the hand be plunged in very cold water, the operation, considered as one of the abstraction of heat merely, is certainly sedative, but considered with reference to the velocity of the caloric in leaving the hand (which by the supposition will be considerable) it must be deemed a stimulant. Again, if the hand be plunged into water of its own temperature, but which is gradually cooled down to a low one; then the operation, considered as an abstraction of heat, has the effect of a sedative; and considered with reference to the velocity of the caloric during abstraction, it has no perceptible effect of any kind.

In addition to those stated in the foregoing theory, one more mode, in which heat may stimulate during abstraction, will be suggested for consideration. May not this fluid, under such circumstances, stimulate indirectly by allowing a fuller energy of the attraction of cohesion between the particles of the solids and fluids of the body to take place? The elasticity of the matter of heat certainly affords the principal antagonist force to this at-

traction; and any circumstance, with respect to the motion of caloric, which should have the consequential operation of bringing the particles composing the human body into more energetic contact, must have, in that view, a stimulant effect.

The view here taken of this subject gains much evidence in its favour by considering the injurious effects produced by caloric whether in excess or defect; it will be found that these depend almost exclusively on its velocity. The impression made by the contact of frozen mercury is said to resemble very much that from the contact of red hot iron: now the effects produced in these cases cannot depend upon the amount of caloric lost or received, because that must be inconsiderable; they must therefore result from the disorganizing operation of the matter of heat, traversing the solids and fluids with great velocity.

It will be found, upon examination, that almost every case which is adduced in proof of the stimulant operation of cold, is such a one as, by its conditions, supposes not a mere abstraction of heat, but an abstraction of that fluid with considerable velocity; while, on the other hand, cases of the gradual and almost imperceptible abstraction of heat are adduced in proof of the sedative operation of cold. Does it not hence appear probable, that the true explanation of the great difference in the effects produced by abstracting heat in various cases, is to be sought in the different velocities of the departing caloric under different circumstances?

There can be no well founded reason for believing that the stimulating effects produced by the motion of caloric are modified by the direction in which such motion is established; on the contrary, it is very probable that the stimulus given to sensation by what is called heat and cold depends so entirely upon the velocity of caloric, that nearly, if not precisely, the same sensation would be produced by the same velocity of that fluid, whether established in the centripetal or centrifugal direction, with respect to the human body; and, taking for granted that the process of refrigeration is regulated by the same laws at all temperatures, if any part of the human body could be cooled down to 16 degrees below zero without injury; such part would be as completely scalded by a sudden plunge into water of a temperature equal to the

animal heat, as it would be under ordinary circumstances by immersion into that fluid at the boiling point.

The peculiar view which is here taken of the effects of heat upon the body attaches great importance to its velocity, and points to the necessity of an exact investigation of the laws which regulate the heating and cooling of bodies; it also supplies a more rational explanation of the manner in which animals endure exposure to high temperatures without injury; and as this question is very particularly connected with the subject of this paper, it may be proper to examine the experiments of Sir Charles Blagden, made to investigate this point. All his experiments went to prove that the human body could bear the contact of air, heated to 30 or 40 degrees above the boiling point of water, without much inconvenience; and the exposure to this great heat never produced a variation of more than a few degrees in the animal temperature. The singular results of the experiments were explained by Sir Charles Blagden, partly from the slow manner in which air communicates its heat, thereby allowing sufficient time for the human body to destroy it—and partly upon the process of evaporation; but his view of the subject will be better understood by his own words.

In the first paper,* Sir Charles Blagden, after enumerating the experiments, has these words: "These experiments therefore prove in the clearest manner, that the body has the power of destroying heat. To speak justly on this subject, we must call it a power of destroying a certain degree of heat, communicated with a certain quickness. Therefore, in estimating the heat which we are capable of resisting, it is necessary to take into consideration, not only what degrees of heat would be communicated to our bodies, if they possessed no resisting power, by the heated body, before the equilibrium of heat was effected, but also what time that heat would take in passing from the heated body into our bodies." He goes on further to say:

"In consequence of this compound limitation of our resisting power, we bear very different degrees of heat in different mediums. The same person who felt no inconvenience from air

* See Philosophical Transactions, vol. lxv. part i. p. 119.

heated to 211 degrees, could not bear quicksilver at 120 degrees, and could just bear spirit of wine at 130 degrees; that is, quicksilver, heated to 120 degrees, furnished more heat for the living powers to destroy than spirit heated to 130 degrees, or air to 211 degrees. And we had in the heated room, where our experiments were made, a striking though familiar instance of the same. All the pieces of metal there, even our watch chains, felt so hot that we could scarcely bear to touch them for a moment, whilst the air from which the metal had derived its heat was only unpleasant."

In the concluding sentences of this quotation, the principle upon which the human body is capable of enduring a high temperature is made manifest. Let the circumstances of the body, when exposed as above described, be examined, and it will appear that the caloric of temperature of the skin, not being in a balanced state with respect to the heat of the air, was constantly receiving additions from that of the latter; but it was also in a minus state with relation to that of the watch chains: why then did the contact of the air, or of the metallic bodies, produce such different effects? Most assuredly because the caloric of temperature issued from the latter bodies with considerable velocity in its tendency to come to a state of rest, whereas this same kind of caloric leaves air comparatively slow, when influenced by the same tendency.

It would therefore appear that these experiments go very much to prove, in confirmation, in part of the theory advocated, that accessions of heat merely, unattended by great velocity of that fluid, have comparatively little effect in stimulating the human system.

But in reviewing the experiments performed in the heated rooms, it will be recollected that the animal temperature varied very little; how is this to be accounted for? The body could bear slow accessions of the heat of temperature from other bodies, without increasing its own heat of temperature; the heat therefore must have changed its state upon entering the human system; it must have assumed the form of latent caloric; that state of this fluid in which it remains at rest, undisturbed by the plus or minus state of the caloric of temperature in surrounding bodies.

It is this capacity which the human body seems to possess under peculiar circumstances of rendering caloric latent, which Sir

Charles Blagden calls a power of destroying heat. Dr. Dobson, however, suggests the explanation on the doctrine of latent heat.

If it be correct to suppose that the caloric became latent in these experiments, it must nevertheless be conceded that the body has a limit to its power of rendering it thus latent; and although the human system may, by an exertion of its preservative power, increase its own capacity for heat, yet this increased capacity must be constantly approaching to a state of saturation; and hence, if the persons, who were the subjects of the experiments, had remained sufficiently long in the heated rooms, there is fair reason to presume, that, after a certain period, the animal temperature would commence to be gradually increased, until it became balanced with that of the surrounding heated medium.

After making these remarks, as giving the most probable explanation of the results of these experiments, it must not be concealed that some of the phenomena observed appear to militate against it. The conversion of the heat of temperature into latent heat, has here been considered as depending upon an exertion of the vital power, not as taking place at the surface and consequent upon the chemical laws of evaporation; yet the slow heating of certain kinds of dead matter, under equal circumstances of exposure to heated air, rather favours the opinion, that possibly the maintaining the equal tenor of the animal temperature in such cases may depend upon the same principle. What these appearances were, will be seen from the following extracts from the experiments.

Sir Charles Blagden mentions his having placed an earthen vessel containing pure water in one of the heated rooms; the temperature being perhaps 20 or 30 degrees above the boiling point; he goes on to say, "In one hour and an half, the pure water was heated to 140 degrees of the thermometer." A little farther on he remarks, "The pure water never came near the boiling point, but continued *stationary* above an hour at a much lower degree." But afterwards he tells us, that, by dropping a small quantity of oil into it, the water at length came to boil very briskly.

Hence it would appear that pure water has a power, under circumstances of exposure to air heated above the boiling point, of

resisting an increase of its temperature beyond a certain degree; for in the above extract it is stated, that the temperature of the water experimented upon became stationary much below the boiling point. This depended, as Sir Charles Blagden remarks, upon the process of evaporation; which must be supposed to have taken place when the water was at the stationary point of temperature, at just such a rate as to be sufficient to render latent any accession of heat as soon as received. The truth of this explanation is rendered almost certain, since, upon the addition of oil, (which acts by preventing evaporation,) the water came at length to boil briskly.

Why, then, it will be asked, if evaporation from water should be sufficient to prevent an increase of temperature beyond a certain point in that fluid, exposed to hot air, may not the evaporation from the human body, under similar exposure, prevent the increase of the animal temperature? More especially, since it is probable, that the same balance of received heat on the one hand, and absorbed heat by evaporation on the other, which takes place with respect to water when exposed to hot air, at a comparatively high temperature, may occur, with respect to the human body, at the common animal heat.

Sir Charles Blagden admits that upon exposure to hot and comparatively dry air, the evaporation must prove a powerful assistant in keeping the body cool; but he cannot believe, that it is alone sufficient to keep it at a uniform temperature; for this reason, which is here given in his own words: "Evaporation can act only in the gross way, and by no means in such nice proportion to the momentary exigencies of the animal, as would be requisite for an exact preservation of its temperature." But it would appear that if the explanation, on the principle of evaporation, was liable to no other difficulty than the one stated as above quoted, it could be satisfactorily made out; for if the dryness of the air should remain the same, then by a kind of physical necessity the evaporation would be directly proportional to the heat of that medium at any moment, and would vary as to its amount upon every alteration, however small, of the heating cause.

But the strong objection to the explanation, exclusively upon the

principle of evaporation, is derived from the experiments detailed in Sir Charles Blagden's first paper; for here he informs us that the body, immersed in hot air loaded with vapour, so far from suffering any process of evaporation from its surface, acted as a condenser of the moisture held by the surrounding air, to such a degree that streams of water poured down its surface on all sides; and under these circumstances also the body preserved its animal temperature; but it must be noticed that the temperature of the heated air was not very considerable. Now it was satisfactorily ascertained that the aqueous fluid which appeared was not sweat, but condensed vapour, since a similar condensation took place upon the surface of a Florence flask filled with cold water, upon being brought into the heated room.

These experiments were indeed attended with curious results; but it is doubtful whether they afford any satisfactory explanation of the manner in which the human system can maintain an uniform animal heat, under circumstances of exposure to comparatively high temperatures. The nature of that power still remains enveloped in difficulty, and will perhaps continue so, until investigated with great diligence by means of other experiments in heated rooms, which should be varied in every possible manner, in order to elicit a consistent explanation.

RECAPITULATION.

1. All agents operative upon the human system are necessarily stimulants.

2. Stimulants differ in the promptitude with which they act. Some act quickly; others comparatively slow. They may be arranged in gradation, according to the promptitude of their action, from the quickest to the slowest in operation.

3. By one sense of the term sedative, a stimulant, slowly operating, is expressed.

4. By another acceptation of this term, the operation of a stimulant relatively defective is conveyed. The term is objected to in this sense; since an agent, which was a sedative at one time, might be a stimulant at another.

5. With a view to its effects on the human system, heat may be considered, 1st, with reference to its relative excess or defect; 2d, with reference to velocity during accession or abstraction. Considered with reference to the former, it is either stimulant or sedative; but with reference to the latter, always stimulant.

6. Quere? May not the abstraction of heat be indirectly stimulant, by allowing a fuller energy of the attraction of cohesion to take place?

7. The stimulating effects of caloric, as far as they depend on motion, are not modified by the direction, whether centripetal or centrifugal, in which such motion is established, with respect to the human body.

8. The view here taken attaches particular importance to the velocity of caloric, in judging of its effects upon the human body: this points to the necessity of investigating carefully the laws which regulate the heating and cooling of bodies.

9. It would appear that the human body is capable of enduring the contact of hot air without raising its own temperature; on account of the slow conducting power of air; on account of evaporation, and of the power which the body seems to possess of increasing its capacity for heat. If it has the last mentioned power, it can exert it to a certain extent only.

10. Water, exposed to heated air, resists an increase of its temperature beyond a certain point, by the evaporation which takes place from its surface.

11. But the maintenance of the equal tenor of animal temperature cannot be explained exclusively on this principle; because the human body exerts the same resistance to an increase of its temperature, when, so far from any evaporation going on from its surface, it acts as a condenser of the vapour held by a surrounding heated medium.

A Case of Chorea successfully treated. Communicated by Dr. Samuel Agnew, of Harrisburg.

On the 20th of June, 1817, I was informed by Mr. M——, that his daughter Sarah, aged between thirteen and fourteen years, was threatened with a return of the disease which had afflicted her in the summer of 1815. It will be proper to state here that I attended this patient on a former occasion, viz. in August and September, 1815, when she was afflicted with symptoms which strikingly resembled chorea. Her symptoms at that time were very similar to what they are at present, except that now they are more violent. I administered at that time the argent. nitrat. in pills, and at the same time bark, having previously prepared the stomach. I also applied an epispastic between the shoulders, and changed the nitrate of silver for other tonics, as myrrh, iron, &c. In the course of seven or eight weeks she became well, and continued to enjoy good health till this spring. In the course of last April, and subsequently, she frequently complained of pain in her stomach, and occasionally in her head, with a sense of fulness; and her appetite was very capricious. She was then, and for many months before, going to school, when from the nature of her studies, and a spirit of emulation, her mind was much engaged, and her body deprived of proper exercise. This state of feeling abated after some time, and she enjoyed tolerable health until about ten or twelve days before her father called on me. For about the space of the time mentioned, she had gradually been getting worse. She was now affected with very irregular motions of her arms, which were involuntary; her legs also moved with much apparent weakness and tottering motion; her speech was considerably affected, her tongue not having power to articulate distinctly; her appetite bad, digestion irregular, though her bowels were not constipated; complexion pale, countenance rather vacant, and her mind considerably affected, rendering her childish, and inattentive to the employments and amusements which had usually occupied her attention. These symptoms forcibly repre-

sented a chorea, which I supposed might *possibly* have for its remote cause an incapacity of the constitution to accomplish the indications of nature at this period of life. Believing that there was a want of energy in the system, and knowing that she had been previously relieved by a similar plan, I began with a view of prostrating the disease by *tonics* and *antispasmodics*. After evacuating the first passages by a cathartic, I prescribed the argent. nitrat. in pills, half a grain morning and evening, and two doses of cinchona in the day. I also advised the cold bath, and exercise in a carriage. These latter, however, were not attended to. The bark purged her considerably; I directed a few drops of laudanum in each dose. Not appearing to better any, on the 26th I applied an epispastic between the shoulders. She was at this time more reduced in strength, and the symptoms in no degree better, perhaps worse. The blister rose well, and ran for several days. No change for the better. The bark was discontinued, and after a day or two the nitrate of silver was omitted, and pills composed of sal. succin. castor and myrrh were substituted in their place. These, however, produced no change; she became rather worse, and on the first of July, in conversation with her father, it had been agreed upon, that I should on the following day introduce a seton back of her neck; I advised at the same time a visit to the Bedford springs. When I called on the second, her mother intimated that it had been suggestsd to her by a neighbouring lady, that the disease might be occasioned by the worms; at the same time enforcing her opinion by a case of one of her own children, which, as usual in the obtrusive vigilance of female quackery, was very similarly affected; and, lo! it turned out, after the child was very low, that it was relieved by medicine administered for the worms, which caused the evacuation of a great multitude. The mother, however, was not unreasonable, submitting it entirely to my judgment. Although I was strongly impressed that worms were not the cause of the disease, yet as the girl was considerably opposed to the introduction of the seton, and as I was aware the medicine which I would give for the worms could produce no injury, but perhaps some benefit, I informed the mother that I would give some calomel powders, and pink root

with senna. Accordingly, on the same day I gave some calomel powders, each of three grains, one to be taken every morning and evening, and the pink root to be used at the same time. On the 5th she appeared considerably better. The medicine had operated pretty freely on her bowels, bringing away a considerable quantity of dark coloured fœtid feces, and but one or two worms. There was a very perceptible alteration in her countenance for the better, and the family were sensible that she had less muscular anomalous action. I felt much encouraged, particularly as I had just been reading Abernethy's Surgical Observations on the Constitutional Origin and Treatment of Local Diseases. I advised the continuance of the calomel, in the quantity of three grains, every morning, for a few days, and a decoction of gentian, senna, orange peel, and cassia, to be given in small doses two or three times a day; so that while it opened the bowels, it would at the same time give tone to the digestive organs.

On the 8th she continued better; her bowels pretty regular, rather open; countenance much improved, and the involuntary motions greatly subsided. I advised exercise every day in a carriage, and the calomel to be taken every other day, with the regular use of the bitters.

10th.—This morning I was quite delighted with the favourable change. The irregularity of motion is entirely gone, the speech distinct, countenance animated, and bowels regular. Her mother informed me that she had been riding yesterday afternoon, and that on her return she had a craving appetite; that she sleeps sound, and appears to feel some interest in the affairs of the family. Continue the calomel every other day, except this day, as her mouth is a little sore; the decoction as heretofore, and exercise.

12th.—She took yesterday a calomel powder, which operated too violently; feels this morning considerable debility, though entirely free of disease. Mouth very slightly affected; no injury, though not desired. I directed her to take 12 grains of calomel every third day, and continue the decoction.

14th.—Took the calomel this morning; has operated moderately. Continues better; is entirely free of involuntary motion, but has considerable debility yet. Continue as before.

17th.—Much better; discontinue calomel. Continue decoction.

20th.—Entirely well; discharged.

This case presents two or three inferences for consideration:—In the first place, it is evident that I cannot claim any merit for its originality; but that the treatment which ultimately succeeded was adopted from accident, and was not the result of ingenious theorising.

In the next place, that although there is neither novelty of speculation, nor even of practice, to many, yet still the record of a practice which succeeded so happily in a disease of awful import, will be beneficial to the young practitioner, and even to the old, where they do not possess the opportunity of perusing systematic works in which the improvements of the medical science are exhibited.

And lastly, that we are not always to expect similar results in cases apparently similar, from the same plan of treatment. For be it remembered, that this patient was on a former occasion relieved by an opposite practice, which was entirely insufficient on the latter occasion. And this inference is confirmed by the result of a case of chorea which I have lately had. A young lad of about 10 years of age, apprentice to the printing business, complained of pain in his lower extremities, which became at last very severe, and deprived him of standing at the desk. After some time his left arm became enfeebled, and he lost the power of holding the type; at length strong convulsive action, very irregular, took place in the left arm and leg, especially the former. He was incapable of holding any thing in that hand, or performing any action under the control of the will; in fact, he had all the irregular, convulsive, anomalous actions of a complete chorea. I tried the plan of treatment which succeeded in the above case for a considerable time, but without the least advantage. I at length resorted to the tonic plan, with the regular use of the cold bath, which after three or four weeks began to lessen the disease; and in the course of about seven weeks he was entirely relieved.

Such are the effects of constitutional diathesis, habits, sex, age, &c. which render the fate of a patient often precarious, and the mind of a physician embarrassed and unhappy.

Aug. 29, 1818.

Remarks on some uncommonly violent cases of "*Colica Pictonum*," by Dr. William N. Luckey, of Lancaster County, Pennsylvania.

> "Inspice nescio quid mihi trepidat *venter* et
> Faucibus exsuperat gravis halitus."
>
> Persii Satiræ iii.

HAVING lately had occasion to attend on some cases of colica pictonum, which, for violence of symptoms, and pertinacity of duration, far exceeded any thing I had ever read on the subject, it may not be altogether uninteresting to lay before you some account of their histories and treatment.

This disease made its appearance about the termination of the month of November, and beginning of December, 1815; nor did it, as is most commonly the case, make its attack on single and scattered cases, but whole families were subjected to its influence. Whoever has had occasion to witness the ingress of a novel and distressing epidemic can well understand, how difficult it is to convey with the pen any thing like an adequate description of its terrors.

The colica pictonum began here with symptoms of violent pain in the region of the umbilicus, accompanied with a sensation of fulness and tension over the abdomen generally, slight feverish heat, hiccough, and flatulent eructations, with a moderate degree of thirst, and a total failure of appetite for food. To these symptoms continued though ineffectual efforts at vomiting succeeded, during which time an anxiety altogether beyond the reach of de-

piction, was apparent in the countenance. All these symptoms were accompanied by a constipation of the bowels almost exceeding credibility.

This description differs little or none from the common description of the disease by nosologists, but when the constipation of the bowels is noted, it will, I think, appear to be altogether different, at least in violence, from the "dry belly ache," or painters' colic.

In the first cases of the disease, under the impression that the violent spasm of the intestinal canal was induced by inflammatory action, blood-letting was employed to the greatest extent, without effectuating any mutation in the violence of the disease, or at all relaxing or subduing the spasm. To diminish the pain, blisters over the umbilical region were resorted to, succeeded by emollient cataplasms, but were equally unavailing. Next Dr. Jennings' steam bath, and the common semicupium were resorted to; but the disease obstinately maintained its position, and rather progressed than retroceded.

The whole history of my tentations for the relief of this obstinate complaint, for some time, consists of a long series of failures. Two ounces of oleum ricini given every two hours, until twelve ounces were taken; enemata of senna, and finally of spiritus terebinthinæ, were equally as unavailing as if one had attempted the solution of a diamond in spring water.

A physician having charge of a considerable number of patients thus affected, after having progressed thus far through the remedies learned from his books, without success, looks around with an almost hopeless eye for assistance. Entirely occluded by the circumstances of his location from having access to an extensive medical library, or the conversation of professional brethren, he feels impelled by his necessities, to "stiffen the sinews, summon up the blood," and make some decided effort for the relief of his patients.

Perplexed and fatigued by the before-mentioned unsuccessful attempts to relieve this intractable disease, recourse was had to tobacco. In one instance an infusion of ℥i. in a quart of water, given at three injections, after being retained fourteen hours, was

returned without any alteration, and unaccompanied by any fœcal matters. Calomel, taken in the quantity of ten grains every second hour, until ʒii. were received and retained, produced no effect. Cream of tartar, aloes, scammony and gamboge, (in doses for which I could hardly obtain credence, were I to state them,) were equally useless. Opium in doses of two or three grains every second hour, evidently increased the distress. The fetid gums were equally ineffectual. Dashing cold water on the extremities, and the injection of great quantities of warm water, were alike useless. The bowels appeared as immoveable as if constituted of metal or leather.

Thus situated, after carefully reviewing my practice, I determined to endeavour, by powerful doses of opium, to release the spasm of the intestines, and to endeavour then to obtain a cathartic operation.

Accordingly, I began by administering large doses of opium, and generally gave ten grains at a dose, every hour, until ℈ii. were received. I was exceedingly rejoiced to find that the symptoms now began to yield, and a few common doses of oleum ricini, or the infusion of senna, would produce the desired effect.

Of forty cases which occurred in my practice, none, except one, proved fatal, after the adoption of the above-mentioned remedy. This patient was attacked by convulsions on the 10th day, and perished on the 22d, without, during the twenty-two days, even having one alvine evacuation. The patient, a potter by trade, became paralytic in the superior extremities, and of course was disabled from pursuing his occupation. I ought to mention that I have frequently in this disease given ten grains of the best Turkey opium every two hours, until ʒi. was received.

REMARKS ON THE ETIOLOGY OF COLICA PICTONUM.

However difficult it may be for us in the present state of medical knowledge, to arrive at a correct pathology of this disease, which appears to have baffled the research of all physicians, yet we ought still to essay it with hope. It is undoubtedly true, in re-

gard to diseases, what the poet Persius has remarked on another topic,

"De nihilo nihil, in nihilum gigni, nil posse reverti."—

This formidable disease has for a length of time been generally attributed to the influence of some metallic poison, and more particularly to lead. From the observations of writers on the diseases of the West Indies, as well as from the remarks of the celebrated Huxham, I should be inclined to doubt the universality of the proposition. Having sedulously attended to the circumstance in my own practice, I have been unable to discover any means by which this destructive agent could be applied to the system, unless it might be derived from the glazing of vessels used for culinary purposes.* But if this were even allowed, the cases by consequence would be sparse and few, whereas in many instances, numbers of a great variety of families were seized at a time.

Huxham, in his treatise, "De Colica Damnonien," (if I recollect aright,) seems inclined to attribute the universal prevalence of the disease to the vast quantity of fruit, more especially apples, which were used in their natural state, and in the form of cyder. Not having the book at present, I am unable to say what kind of reasoning, or what mode of treatment, was adopted by him.

From the manner in which Dr. Mosely speaks of this disease, it is very evident that he considers it as a disease of no great danger, as he tells us, "When the disease comes on, *if the bowels*

* This was undoubtedly the case. We saw four cases in a family, in the neighbourhood of Dr. Luckey, in the fall of 1815. The disease in these instances was traced to the pernicious influence of lead received into the system, by the use of apple-butter, kept in earthen vessels very imperfectly glazed. On examining one of the crocks, out of which the greater part of the apple-butter had already been used, a thin crust of acetite of lead was found covering its internal surface. The frequency of these cases, during the period mentioned by Dr. Luckey, arose from the country people, in the vicinity of Elisabeth-town, putting up their apple-butter during that fall in badly glazed vessels, got from a neighbouring pottery. The acetic acid contained in the apple-butter, uniting with the glazure of the crocks, (composed of red lead,) produced the acetite of lead, which, being received into the system with the apple-butter, gave rise to these cases. Ed.

are costive, let a dose of manna, or cream of tartar, be taken, or what answers better, if it can be procured, a table spoonful of castor oil; if one spoonful does not operate in a few hours, another spoonful is to be taken, and repeated every four hours until it does." You may find, I believe, some remarks on this disease in "Hillary on Barbadoes," which I have not an opportunity of referring to. Sir George Baker has also a paper in the Philosophical Transactions, which I have not seen, although I expect it cannot contain any thing uncommon or important, as Mosely speaks of it, without making use of any of his remarks.

From what I have seen of the disease, I feel inclined to ask the question,

> ——————"Calido sub pectore mascula bilis
> Intumuit quam non extinxerit urna Cicutæ?"
>
> *Persius.*

It appears to me that the disease in question owes the major part, if not the whole of its violence, to the impression of some agent on the liver and biliary system, either immediately by repressing the perspiration, or directly on that organ in some manner with which we are unacquainted. This opinion is countenanced by the great torpor of the bowels, anxiety, &c.

I have trespassed on your patience by my prolixity, and must ask your forbearance for the manner in which these remarks are thrown together; a man who has been long and actively engaged in the practice of medicine, can scarcely be expected to write with excellence. I was inclined to send you these remarks, as I have felt the distress which arises from being unable to derive any assistance from books on the subject, and with a hope that they may be of use to others. To conclude, I may say in the words of Cicero: truly

"*Edidi quæ potui,* non *et volui* sed ut me temporis (ac facultatis) angustiæ cæderunt."

I will also take the liberty to enclose you the history and treatment of a very violent case of disease, which has within the last

six years been named the " Pneumonia Typhoides." This case came under my observation in June last.

A waggoner arrived in this place about noon, and after unharnessing his horses, repaired to an adjacent tavern. He had scarce entered the house ere he was affected by a slight chill, which did not last more than fifteen minutes, when it was succeeded by an apparently violent fever. The pulse was weak and feeble; the breathing difficult and sighing; the eyes suffused, fixed, and glassy; the tongue dry, blackish, and covered with a crust, a cough strangulating, and with no expectoration, attended by pains of the head and bowels.

To these symptoms succeeded in a few minutes a most distressing delirium. The patient sat upright in his bed; his eyes immoveably fixed, shouting and raving terribly, making at the same time most violent exertions with his arms, so that he was with difficulty retained in bed. As the fits of exertion would cease, his strength was very much prostrated, and he appeared utterly powerless; and was evidently sinking in such a manner as to lead to the conclusion that he would soon be out of reach. Being forced to make some decided effort for his relief, I prescribed and gave him 120 drops of tincturæ opii in ℥iv. of French brandy, with orders to repeat the brandy every hour, and the tinctura opii every fifth hour, in the dose of 100 drops. In this manner 92 ounces of the best brandy were taken. The opium failed to produce any other effect than that of ease from the most distressing symptoms, though no sleep. In twenty-one hours the patient had taken ℥ss. of the tincturæ opii. This treatment was persevered in for twelve hours longer, and the disease subsided. His recovery was as rapid as his disease had been violent in its attack. It is also worth noticing that the patient was remarkable for his temperate habits.

I have subjoined this case, not as a precedent, but as showing how far it may be at times necessary to increase the doses of our medicines. It is my opinion, that there is yet great room for improvement in this particular in our profession.

Three Cases of Cystirrhœa. Communicated by G. French, M. D. of Fredericksburgh, Virginia.

CASE I.

On the 21st of December, 1799, I was sent for to visit Mrs. S. 12 miles from this place, aged about 40, of a lax and delicate habit. She is one of the first families in this country, accustomed to live generously, and been the mother of twenty-one children. She was confined to the chamber, but mostly on the bed, and complained of excessive pain, before, after, and during the discharge of urine; a constant pain under the os pubis, shooting across towards both the ossa illia, and extending as high as the umbilicus, which occasioned a degree of nausea, a bearing down of the uterus, such as was common when the placenta was expelled, as she expressed herself. She complained of frequent tenesmus, and a desire to discharge urine, almost every ten minutes; had no stoppage in discharging it, but some difficulty, when the pain was violent. It deposited a quantity of sediment, something similar to mucus mixed with purulent matter, of a ropy appearance. The quantity of urine discharged in twenty-four hours was more than when in good health.

The commencement of this disease was in the preceding July, when in the upper country, about 150 miles from this place. She applied for medical assistance, and the gentleman who attended, supposed it was a case of calculi in the bladder. He prescribed lithontriptics, with some other medicine which I do not know: at first they gave some ease, but after a short time, every symptom was aggravated by them. Before her return, she had discontinued all medicine for two weeks, and was reduced considerably. Menses regular, skin hot and dry, pulse 98, full and hard, face flushed, pain in the head, very costive, and slight pain in the back: twelve ounces of blood were taken from the arm, and a cathartic of ol. ric. was directed; barley water was ordered, with gum ara-

bic, for drink; tinct. opii. gtt. xl. were recommended to be taken at night.

On the first day of January, 1800, I visited her a second time, and pointed out the necessity of examining the uterus per vaginam, and also the introduction of a sound into the bladder.

Mrs. S. being a sensible woman, and having a numerous family, to which she was much attached, readily consented to every thing I thought would be necessary to ascertain what complaint she laboured under, however disagreeable it might be to her feelings.

On examining the uterus, I found it was in its proper state, and appeared to be nowise diseased. A sound was introduced into the bladder with great ease, and a careful search was made, but no stone could be discovered. She complained during the examination that the instrument hurt her when in the bladder. No thickening or contraction was discovered. After having every opportunity of examining with some degree of certainty, the situation of the uterus and bladder, I was at no loss in determining it a well marked case of cystirrhœa.

Injections were recommended; such as Mr. Brown, surgeon in Mussleburgh, has so judiciously prescribed, in a case published in Doctor Duncan's Medical Commentaries, vol. 10. The linseed oil and lime water occasioned such excessive pain, that it was discontinued, after having used it two days; that with sulphas zinci to the extent of 35 grains with emulsio, gum tragac. and two drachms of watery solution of opium, gave relief for some time. These injections were used from the first of January till the 24th of March. During this time mercury internally and externally was used. Bark and its different preparations, steel, cicuta, and uva ursi, were also used. The last had the best effect for three weeks; after that time it produced no relief. Opiates were given every night: emollient, balsamic, and a great variety of other medicines, were prescribed; but as they did not produce any good effect, in checking the progress of the disease, they are not worth enumerating. The regimen recommended was fowls boiled and roasted; veal and mutton in small quantities. Barley, rice, potatoes, sago, tapioca; calves' feet and hartshorn jellies; soups, milk, coffee, chocolate, and fruits of different kinds, were used alternately, her

drink consisted of barley and rice water, with gum arabic dissolved in it; flaxseed tea, thin tapioca, runnet whey, wine and water, and cold water was sometimes permitted to be drunk.

After such a length of time, and every medicine prescribed had been taken with great exactness; regimen and drink strictly attended to, and also the different injections had been used, and borne by her with great fortitude; and to my great uneasiness and disappointment, every symptom was more aggravated; the coats of the bladder were now so exquisitely sensible, that the stimulus of the smallest quantity of urine occasioned the most excruciating pain. Her cries were heard all over the house; the sediment was now in larger quantities than before, six ounces of urine deposited one ounce and a half of sediment by measure, which was sometimes tinged with red, at other times with yellow, and sometimes with white. Great pain through the urethra and vagina. The abdominal muscles were much affected, with a great degree of tension and spasm; to relieve which, fomentations were ordered, and tinct. opii ʒij. spts. vin. camp. ℥ij. spts. c. c. ʒi. m. were applied all over the abdomen. In a few days the tension and spasm were entirely removed. Her countenance was sallow, she was much debilitated, and confined to bed:—During the night, cold sweats with some cough and slight attacks of dyarrhoea; pulse 107, soft and feeble.

Having tried the different medicines recommended by the best authors and practitioners, without any lasting effect, and taking a view of her former and present situation, I gave it up as a desperate or lost case.

This determined me to leave the old beaten track and try something new: a practice no less bold than hazardous, the result of which, however, justified the innovation. Having formerly been subject in the summer season, to have my under lip excoriated and slightly ulcerated: by washing it with a solution of nitras potassæ, in a few days it always got well.

Being convinced that the coats of the bladder were excoriated, and that slight ulcerations had also taken place, I supposed the nitras potassæ might have the same effect when thrown into the bladder, as it would then be applied immediately to the parts

affected. I imagined that when thrown in, it would give considerable pain; but in this I was disappointed, for it gave no more uneasiness than the former injections, until the cure was nearly completed; then it occasioned so much pain, that the quantity was reduced one third. The injections with nitras potassæ commenced the 24th day of March; after they were used for eighteen days, the pain before and after the discharge of urine, was not so acute; the tenesmus was completely relieved; she could retain her urine longer, the night sweats not so copious, and a shorter duration. On the 10th of May, every symptom was removed excepting some degree of weight in the region of the pubes, and some sediment in the urine, of a whitish, transparent colour. Diet and drink recommended as formerly when in good health. She was directed to ride out in a carriage every day, when the weather was good. June the 8th, to my extreme satisfaction, I found that every symptom of the disease was removed. I recommended Mrs. S——— to go up to the mountains, and use the cold bath; the shower bath she had used before. Some of the ingredients for injection, with others, were recommended to be taken along with her, in case any of the symptoms should return; but they were not used. The injections were administered by a servant maid. The instrument used was a female catheter, mounted with a bladder. The injections used from the 24th of March till the 23d of April were, mucilage gum tragac. ℥xij. nitras potassæ, ʒi. tinct. opii ℥ss. ♏. Four ounces were directed to be used every morning, noon, and night; and when the pain was violent, they were given in the night. From the 23d of April, until they were discontinued, tinct. opii camph. ʒiss. was substituted in the place of the tinct. opii. There was two thirds more of the opium in it than is recommended in the Edinburgh Dispensatory. The change was owing to a mistake of one of the young gentlemen in making up the injection. I did not discover it to Mrs. S———; but she thought that the last bottle of injection had been of more service than any of the former. A mixture occasionally was taken, when the pain was severe, during the whole of the disease, composed of mucilage gum arab. ℥xij. tinct. opii. camph. ʒij. carbonas potassæ, ʒij. M. Two table spoonfuls were directed to be taken

every two or three hours. Opiates were increased from one grain to four, from December till May; when they were discontinued. They occasioned a costive habit; to relieve which, a great variety of cathartics were taken; but none of them agreed so well with my patient as pulv. rhæi, gr. xxv. mag. alb. ℈i. carbonas potassæ, gr. xx. spts. vol. arom. gtt. xxv. M. This was to be taken occasionally in the morning, mixed with cold water and sugar, or mint tea. It generally operated twice the first day, and kept the body moderately open for several days. Menses were regular during her confinement, but had no effect in aggravating the symptoms, so long as they continued. An infusion of bark was sometimes given, when using the last injections.

On the 25th of September, 1802, Mrs. S—— was delivered of a fine boy; I had some difficulty in extracting the placenta, but she recovered as soon as formerly, and enjoyed a good state of health from the 8th of June, 1800, until July the 11th, 1803, which was upwards of three years, when she had a second attack. The former injections were directed and used until the 25th of the same month, on which day they were discontinued; the disease being removed.

On the 20th of October following, some of the symptoms having returned, the injections were again prescribed. At this time Mrs. S——— was pregnant, and continued so until the 27th of December, when a miscarriage took place. During the time of her pregnancy, the injections were used occasionally, and always relieved the complaint; but when they were discontinued the disease soon made its appearance again.

After abortion took place, bark in powder, extract and decoction, with columb. was directed: when the pain was troublesome, a mixture of emul. gum arab. carbonas potassæ, tinct. opii camph. were directed to be taken through the day, and sometimes anodynes at night: when the mixture was not taken, sal soda to the extent of gr. xij. with sap. hisp. made into pills, was directed to be taken, morning, noon, and night. This course was continued from the 27th December, till the middle of March following. The spirits of Mrs. S——— were much depressed, from a fear that she might suffer as much as she had in the year 1800; al-

though the present attack was slight, compared to the former. Wishing to satisfy myself fully as to her unfortunate situation, I recommended a consultation with Dr. William Baynham, a gentleman of distinguished eminence in his profession.

On the 15th March we met, and I stated to him all the symptoms of her former and present disease; the great variety of medicines that had been prescribed, the injections which were used, and the effects they had produced. To remove every doubt as to the existence of a stone, we sounded the bladder, but no stone could be felt. Dr. B. agreed with me in opinion as to the disease, and recommended to Mrs. S——— a continuance of the injections, observing that he had no doubt but they would produce the same happy effects that they had in her first attack; having certainly cured the former, and given relief in the latter.

It was highly gratifying to me, to find that a gentleman of doctor B.'s well known abilities as a physician, should coincide with me in the mode of treatment of this perplexing case.

In the month of May following, Mrs. S——— again became pregnant, and continued to use the injections and anodynes: After the fourth month of pregnancy, the fundus uteri having begun to rise above the brim of the pelvis, the injections were discontinued, the disease being removed. Sal soda or carbonas potassæ were frequently taken in pills, which proved to be of considerable service: a variety of other medicines were also taken during gestation, but I am clearly of opinion, they were of no benefit to my patient. On the 7th of February I delivered Mrs. S——— of a fine child, and from its appearance no one would have imagined that the mother had laboured under any disease. Sixteen days after her delivery she complained of some uneasiness in the region of the bladder: the urine deposited a quantity of sediment of a pellicle appearance; the injections were prescribed and continued till the 29th of April; since that period Mrs. S—— has enjoyed a good state of health, and is of a fuller habit of body than she had ever been before.

CASE II.

November the 1st, 1806, I was requested to attend Mr. M—, aged thirty-six, of a slender make and dark complexion. He had generally enjoyed good health; and having a handsome fortune, he was enabled to live generously, and had been accustomed to take moderate exercise. The urethra was very much tumefied and tender. The urine deposited a quantity of sediment like mucus and pus mixed together, of a fibrous appearance. He complained of great pain in the region of the bladder, and of constant inclination to make water, which was attended with great pain, both before and after the discharge, and during the night the discharge was more severe. He had a constant fever with frequent chills, head-ach, costiveness, tenesmus extremely troublesome, tongue furred, constant thirst, and appetite much impaired. He was considerably reduced, his spirits much impressed; was confined to his chamber, but mostly to the bed.

The commencement of his disease took place on the 20th of October preceding. Having attended two well marked cases of the same kind, I had no doubts in pronouncing it another case of cystirrhœa. Ole. ric. was directed to be taken; to drink barley water with gum arabic in it. Four ounces of injection were directed to be thrown into the urinary bladder, three times a-day, composed of mucilage gum tragac. nitras potassæ, tinct. opii camph. of the same strength as those directed in the case of Mrs. S———.

Emul. gum arabic ℥v. spts. eth. nit. ℥i. tart. antimon. gr. ij. m. A table spoonful was ordered to be taken in a wine glassful of balm tea every two hours. This was continued for twelve days, when the fever disappeared. Sal soda to the extent of gr. xij. sap. hisp. gr. vi. made into pills and given, morning, noon, and night.

Between each dose of the pills, ʒss. of bark, mixed with wine and water, was taken for ten days, when they were discontinued. Spts. lavend. comp. ℥i. carbonas potassæ ʒiss. m. a tea spoonful with water and sugar was given every three hours. Opiates were

administered for thirty nights. To relieve costiveness, pulv. rhæ. gr. xxx. mag. all. ℈iss. carbonas potassæ gr. xx. spts. vol. arom. gtt. xx. m. were given occasionally in water and sugar.

On the 13th, 14th, and 15th days after the injection was used, a larger discharge of sediment took place, than at any other period of the disease: it was tinged with blood, and a considerable quantity of matter was discharged after the urine had ceased flowing. At this time he complained of a lightness in the hypogastric region, and also through the whole of the pelvis: to relieve those distressing symptoms, fomentations were recommended three times a-day, and liniment vol. anod. camph. were rubbed all over the parts affected, after each fomentation. So long as the fever lasted, tea with bread, hartshorn jelly, tapioca, sago and soup, were his principal diet. After the fever was removed, tea, coffee, bread and fresh butter, were recommended for breakfast: Fowls roasted or boiled, veal, mutton, rice, potatoes, cabbage, and sometimes poach eggs for dinner, were used alternately. His constant drink for the first twenty days was barley water, with gum arab. after that period he drank wine and water, and cold water.

After having used the injections for thirty-seven days, they, and all other medicines, were discontinued, every symptom of the disease having been removed.

Mr. M——— has enjoyed a good state of health ever since.

The injections were administered by my assistant Dr. Wacker, and the instrument used was a female catheter, mounted with a bladder. He never found any difficulty in introducing the instrument.

I found in this case, as well as in that of Mrs. S———, that the sal soda, and potassæ, were of great service, and that the tinct. opii camph. was much preferable to the tinct. opii or the solution of gum opii: I must be permitted to suppose that it was entirely owing to the effects of the gum camphor.

If ever I should attend another similar case, I shall recommend larger quantities of camphor.

CASE III.

In the year 1801, I attended a free negro man, whose case was so similar to that of Mr. M——, that I think a minute statement of it not necessary. He was thirty years old, and of an athletic make. Before I was consulted he had employed an African negro, who persuaded him that he was poisoned, and pretended that he had a remedy.

He was under the care of this African for eight weeks, during which time the symptoms became more violent. He at last discovered the ignorance of the person he had employed, and became anxious to be taken under my care.

For the first fourteen days, different directions were recommended, also medicines internally; but no alteration for the better took place. From that time injections made of mucilage gum tragac. ℥xij. nitras potassæ ʒi. tinct. opii camph. ℥iss. m. four ounces were injected into the urinary bladder four times a-day for thirty days, when they were discontinued, and a perfect cure was accomplished. During the continuance of the last injections, no medicine was given but opiates at night. His diet was whatever he could procure for himself, except what was sent from my table. His drink was barley water with gum arabic in it. He enjoyed a good state of health for upwards of three years, when he caught the small-pox in the natural way, and died of that disease in a few days.

It was once my intention to have given at large my theory on the mode of treatment which I have established in the three before described cases; but as I should have differed so widely from several practitioners in their speculative theory, I have thought it the most prudent step, to state the simple facts, and leave it to those whose abilities are far superior to mine.

A Memoir on Contagion.

We need offer no apology, we hope, for introducing into the Recorder the following extract from an interesting memoir on contagion, by Dr. N. Potter, professor of the theory and practice of medicine in the university of Maryland. We have read it with much interest; and we believe it will be equally acceptable to the readers of the Recorder. It forms an irresistible body of evidence in proof of the indigenous origin of our yellow fever, as well as a lucid and ingenious exposition of the legitimate and distinctive characters of *contagion* and *infection*.

Et meæ si quid loquar audiendum vocis accedet bona pars.

Ed.

A Memoir on Contagion, more especially as it respects the Yellow Fever.

[Read in convention of the Medical and Chirurgical Faculty of Maryland, on the 3d of June, 1817.]

Mr. President, and Gentlemen,

I rise to discharge an obligation justly due to this convention, one which I was precluded from fulfilling by corporeal indisposition on a former occasion. I am sensible, that life, strength, and glowing colours will be wanted in the execution, and that an honest zeal is my only qualification, commensurate with the importance of the appointment.

Perhaps it may be asked, why I should have selected this polemic subject, at so late a period of the long agitated controversy. To this suggestion I would answer, I had cherished a hope that the question had been consigned to rest, never to be revived.

To my equal astonishment and regret, it is manifest from opinions both public and private at home, as well as from high literary sources abroad, that there is still not only a *La Vendee* in our own country, but a host abroad, arrayed in opposition to that opinion, which alone would seem consonant to sound philosophy. *The Army Medical Board* of Great Britain, professing to have bestowed on this subject the most profound consideration, has deliberately and solemnly adjudged the yellow fever a *contagious* disease, and consequently capable of being communicated by commercial intercourse. In this decision we feel a more than ordinary solicitude, because we are persuaded, that the consequences which must flow from its admission are so fraught with calamity, both public and private, that neither the philosopher nor the philanthropist can contemplate them without emotion. Indeed it becomes the more momentous from the reflection, that it emanates from an honourable and learned body, whose distinguished rank in the republic of medicine may awe surrounding nations into admiration and submission. While we lament that the country which gave birth to a Newton, a Bacon, and a Shakspeare, that has "waked to ecstacy the living lyre" on every other theme, should have sounded a discord on this tender string, we can but view the expression of such a sentiment as one of those solecisms in science which belongs exclusively to the fatality of the subject.

At the very threshold of the investigation, we are to encounter an impediment, which, although when met with energy, will be found to have nothing to boast but its antiquity, has become formidable from the tyranny of custom. That sentiment which has been imbibed as an elementary principle of our education, which has "grown with our growth, and strengthened with our strength," which has been sanctioned by legislative authority throughout the whole maritime world, would require the sagacity of a Socrates and the eloquence of a Burke to combat it with success. Independent of this despotism of education, and all the prejudice it must inspire, the pride of nations has rivetted the chains it has forged. Pestilence has been viewed as an alien to all the world; it has been disdained as a national stain, and chased from every clime. When it has invaded the European or American

continent, it has been imputed to the eastern or southern sections of the globe, whose indolent and benighted inhabitants had neither the energy, nor the science to repel the illiberal charge. The first settlers of the West India islands charged the yellow fever to Siam, and as one error begets another, we have retorted the futile imputation upon them.

> " No nation owns it in the first degree,
> But thinks his neighbour further gone than he;
> Ev'n those who dwell beneath its very zone
> Or never felt its rage or never own;
> But where the extreme of vice was ne'er agreed,
> Ask where's the North; at York, 'tis on the Tweed,
> In Scotland, at the Orcades: and there,
> At Greenland, Zembla, or the Lord knows where."

Since the doctrine of imported contagion has appeared questionable to the more scrutinizing eye of modern philosophy, although we witness many efforts to refute it, we can but lament, that writers have generally* restricted their views to a sphere too contracted, to reflect that light, of which the subject is susceptible. Instead of appealing to the history of all warm latitudes, which originate this disease necessarily, they have limited themselves to the strict letter of medicine; and few of them have ventured beyond the precincts of their own cities. It may be laid down as a general position, that all countries in tropical latitudes, and all others liable to tropical seasons, are destined to suffer that form of pestilence, now so notorious under the appellation of yellow fever. If the histories of such places had been faithfully transmitted, we should invariably find pestilence as one of their natural productions. The colonial histories of our continent, are replete with facts to prove that this disease is the offspring of putrefaction, communicable only through the medium of the atmosphere. We will avail ourselves of some of them, as auxiliaries to our medical histories; but we will first appeal to Hippocrates,

* The most expanded and liberal view of this important subject, has been given by a gentleman, who, although he was not educated a physician, has contributed more than his quota to the history of epidemics.

Vide Webster on Pestilence.

the most ancient authority in medicine. On various occasions in his writings, we find depicted all the characters of that fell disease, which has since inflicted so deep a wound on our own country. This illustrious Greek viewed the disease he so well describes, in the mild climate of his native soil, almost in the parallel of latitude in which we live. He speaks a language without disguise, susceptible of but one interpretation. The *tout ensemble* of his faithful picture portrays the disease in colours as glowing as those of Chisholm or Rush. He enumerates the more prominent symptoms, under the following appellations: καυσος, *the ardent fever;* τυφος, *a stupor or coma;* φρενιτις, *an inflammation of the brain or its investing membranes;* ικτερος, *a yellowness of the skin*, and caps the climax of the malignant picture by the words μελανα εμεσον, black vomit, and μελανων εμεσον, the vomiting of black matter. "*In burning fevers,*" *says Hippocrates,* "*a yellowness of the skin on the fifth day, especially accompanied by a singultus, is a sign of great malignity.*"* In the first section of his first book, on prognosis, he states in the most explicit terms "*that the black vomit is a most dangerous symptom.*"† He ranks the disease to which he attaches these pathognomonic signs among the endemics of Greece, and on this, as well as all other occasions, attributes its origin to the uncommon heat of the seasons. It would be superfluous to adduce other proofs, (with which his works abound,) for the most fastidious nosologist cannot improve upon his emphatical definitions. The visionary idea of an exotic origin never entered his imagination. He does not invidiously impute the pestilential visitation to other nations; he does not stigmatize the Phœnicians with having introduced it by commercial intercourse; nor does he insinuate that it was introduced through the Bosphorus, or imported from the islands of the Archipelago.

As no well marked distinction between miasmatic and other epidemics was taken till it was suggested by Lancisi, we will only recur to more ancient authorities, when we come to examine the

* Εν τοισι καυσοισιν εαν επιγενηται ικτερος και λυξη πεμπταιω εοντι, θανατωδες υποφορη λαμβανονται.

† Sect. 9th, Book on Crises.

doctrine of contagion with a view to its pathology. He bears strong witness in our favour, in his description of the bilious pestilential epidemic that ravaged Rome, in 1695. He has left on record the most explicit declaration that the fever was limited to those parts of the city that were built on factitious materials, like artificial grounds of parts of our cities at this day; and declares that the hills, only a few paces distant, were exempted from the disease; he finishes his description of this mortal fever, by expressing his astonishment, that so short a distance could have made so essential a difference in the air. He ascribes the disease solely to the effluvia extricated from the low grounds acted upon by unusual heat.*

Baglivi is equally impressive in his history of the same fever; they both describe its malignity, and equally lament its fatality. I would ask no greater favour of the abettors of the contagion of yellow fever, or those who are sceptical on this question, than a fair and candid appeal to those writers.†

Bartholine describes a malignant fever that wore the very features of the yellow fever of our country; one that (on account of its mortality) struck a panic through the city of Copenhagen, in 1652, and caused it to be deserted. He states that he dissected a great number of bodies, and in every case found the stomach and duodenum highly inflamed or gangrenous. He does not ascribe it to any extraneous source, but refers it to putrefaction occasioned by uncommon heat; "to something rotten in the state of Denmark." His description concludes with this emphatic expression: "Its mortality was arrested only by frost."‡

Forestus is no less explicit in his description of a malignant bilious fever which infested the city of Delft for several years in succession. This fever generally terminated in death on the fifth day. It was occasioned, says this faithful historian, "*from the unusual heat of successive summers, and because the city was surrounded by stagnant waters.*"

* Lancisi de Noxiis Paludum.

† Baglivi Opera Omnia, page 157, et aliis locis.

‡ Thomæ Bartholini Historiarum anatomicarum rariorum.

Silvius De Le Boe describes a most inveterate bilious remitting fever that raged at Leyden, in 1669. He attributes it to the uncommon heat of summer and autumn, a paucity of rain, and an almost uninterrupted calm, by which the waters in the canals were nearly evaporated, and consequently stagnated. The air was so pestilential, says De Le Boe, that two-thirds of the principal inhabitants perished.*

Cleghorn, in his account of the diseases of Minorca, describes a disease similar in character; obstinate, and sometimes intractable; but views it as the endemic of the island, the offspring of marsh effluvia, in common with other bilious fevers, all of which vary in degree according to the season.

Sir John Pringle, treating of the fevers that vexed the British army in Flanders, remarks: "the remitting fever was more general and fatal after the extreme heat of the summers 1743 and 1747." The same distinguished writer, speaking of the fever that infested the troops encamped near the inundations in Dutch Brabant, in 1748, adds "*in the greatest heat of the weather, and rage of the distemper, most of the fevers answered the description of the causus of the ancients.*"

It will be almost superfluous to mention the island of Zealand, so memorable for its disastrous influence on the British army, in 1809. In Doctor Blane's letter† to the physician-general, dated Middleburgh, October 3d, of that year, and presented to parliament in February, 1810, we find the following melancholy account: "It appears from the last general weekly return, that nearly two-thirds of the whole numerical strength of the army is incapable of duty; the mortality of the last three weeks has been about a thousand." This fever never has been viewed in any other character than as the endemic of the island, affecting strangers almost exclusively. About twelve thousand of those troops were remanded to England from Walcheren, and the sick were deposited in the hospitals, but no disease was communicated to those with whom they held constant intercourse, either on

* Silvius De Le Boe. Prax. Med. Tract. X.

† Documents presented to parliament in February, 1810.

board the transports on the passage, or on shore afterwards. The malignant character of this fever, derived solely from the inquinated atmosphere of the island, is too notorious to require a comment. It may be asked, were all these diseases genuine yellow fever? To this it may be answered, that if they were not all of equal grade, they were from the same remote cause. It is physically impossible that every one of them should have maintained the same grade. The difference in the force of the remote cause, as well as the previous condition of the subject, invariably modify the character of diseases, more especially the whole family of miasmatic fevers. It is incontestably established by each of them, that they were extraordinary in virulence, transcending, in malignity, the ordinary diseases of the same places, as far as the yellow fever of our country transcends the common remittent.

Let us now appeal to the civil history of our own country to prove the indigenous origin of bilious pestilence. We have established the fact, that it has originated even in cooler latitudes than our own, and the colonial histories of our continent abound with proofs that it is of a texture solely domestic. They claim our highest and most respectful attention, because, having no favourite hypothesis to support, and enlisted under the banners of no party, they have left the naked facts. If those who recorded the facts were influenced by any prepossession, it must have inclined to the hypothesis of contagion, for in those days it was as much a popular superstition as the belief in witchcraft or ghosts, and rests on as baseless a foundation.

It is very remarkable, that the first accounts of pestilential epidemics in our country should have been noted so specially by all the historians of the times, and that they should correspond in all essential points. It is manifest, that the series of epidemics at that period were so pestilential, and so general, that a deep solicitude was felt by every one. So notorious were the facts, and so disastrous the consequences, that they became matters of record in every colony of which there is a history extant. Georges in his History of New-England, Hutchinson in that of Massachusetts, and Purchas in his Pilgrims, as well as Smithe, concur in recording the more prominent facts of the series of mortal epidemics that

commenced about the year 1607, and continued about thirteen or fourteen years, to the great annoyance of the colonists, as well as the discouragement of those who were disposed to emigrate to the new world. In 1607 and 1608 it depopulated a company of Englishmen, who had settled in Massachusetts under letters patent from James the first. It was denominated the *calenture*, or burning fever, an ancient appellation synonymous with causus, and seems to have pervaded all the colonies in the most virulent degree. Purchas states, "that captain Dormer, an Englishman, who had arrived in America two years before, passed the winter 1618—19 in Minichigan, an Indian settlement on the northern coast: in the May following he sailed along the coast, bound to Virginia, and landed at several places where he had been the year before. He found many of the Indian towns depopulated; in others only a few of the natives had survived, but not one free from sickness." He adds, "the disease must have been the plague, for I perceived the sores of some who had escaped."

In Belknap's Life of Fernando Georges, a fact is stated that indicates a strong feature in the character of bilious pestilence, but is hostile, in the extreme, to the doctrine of contagion, which is no respecter of person, which spares no nation, sex, or condition, unless those on whom it has previously made its impression. "Richard Vines and his associates," says the historian, "remained among the natives during the whole pestilence, but were exempted; not one of them had the disease." This assertion cannot be predicated of any contagious fever, and can only be explained on the principle of habitual impression. Vines and his companions, (who seem to have been rovers by profession,) had no doubt been accustomed to breathe a contaminated atmosphere, and to this alone they owe their immunity.

Mr. Webster, in his learned work on pestilence, quotes a passage from the Magnalia of Massachusetts, which corroborates the narrations of the cotemporary historians, but is more explicit as to the character of the epidemics of the times. Gookin, who recorded the facts, "declares that he conversed with several of the natives who escaped this mortal fever, and that they unanimously declared, "*that the sick, but more especially the dead, were remark-*

ably yellow." As neither of them understood the language of the other, the Indians resorted to comparison to render themselves intelligible to their visiters. They produced some articles of clothing of *a yellow colour*, and by various gestures imparted their impressions.

In Prince's chronology, another history of deserved reputation, we find a conclusive fact respecting the same fatal fever. He describes it as "*terminating in death from hæmorrhages*, especially from the nose."

Can there be a doubt respecting the character of this pestilential disease? The most irresistible evidence arises from the repeated representations of the historians of the times. Purchas every where ascribes the difficulties and misfortunes of the first settlers, in part, to the extraordinary condition of the atmosphere which was not more congenial to animal than vegetable life. He expressly declares, "*that the foul air at Jamestown was the cause of the mortality among the colonists.*" This connexion between scarcity, sometimes famine and pestilence, at some period, marks the history of all nations.

The celebrated Smithe, in his work entitled "*A General History of Virginia, New England, and the Summer Isles, by captain John Smithe, sometimes governor of these countries, and admiral of New England, commencing* 1548, *and ending* 1626," is as unequivocal on this subject, as any writer, not exclusively devoted to the science of medicine, could have been. In the autumn of 1622, the deplorable state of this settlement, arising from the memorable Indian massacre in March of that year, from the scarcity of provisions, and the almost depopulated state of the colony from a pestilential fever, occasioned a general despondence among the survivors. So well founded and clamorous were their complaints, that all the energies and address of their intrepid captain were required to reconcile them to their forlorn condition. In reply to the remonstrances of his disaffected company, after enumerating the other causes of their troubles, he finishes his pathetic appeal by imputing the pestilence that had proved so fatal, to elementary causes, neither to be foreseen nor controlled. These are his words: "*You might as well renounce the kingdom of Great*

Britain, because the fens and marshes are unhealthy and barren." The complaints of men who had abandoned their native country, and all that was dear to them, might well become grievous when it was manifest that this pestilential visitation was a national calamity, that had swept all the settlements, and had even invaded the aborigines. Hutchinson states, "that 30,000 of the Massachusetts' tribe of Indians were reduced to 300." Purchas declares that "the emigrants to Virginia, in 1619—20—21, amounted to 3570, in 42 sail of transports. There were 600 in the colony before, making 4170; of these, 349 perished in the massacre of 1622, which leaves 3821. In 1624, only 1800 were living." Among the causes already enumerated there must have been some one more prominent than the rest, to account for the unprecedented defalcation of the colonists. The pestilential state of the atmosphere furnishes the only rational solution of this incontrovertible fact.

From these authorities, *and from every other well attested fact, the inevitable conclusion must flow, that as the* yellow fever did then prevail endemically, between the 41st and 44th degrees of latitude, it can again, and necessarily must originate there, or in any other place where the mercury of Fahrenheit's thermometer rises to 85°, provided moisture, and the materials of putrefaction, be present. If there be any so deeply infatuated with the visions of contagion, as to dream of importation into those infant colonies, let them reflect that in 1618, and for many years subsequent to these reiterated pestilential epidemics, there was no communication between this country and the West Indies: not one of the islands was then settled, except a small part of Hispaniola, with which the colonists had no intercourse.

From the cessation of this pestilential series to the year 1741, the medical history of our country, (especially of Massachusetts, New-York, Pennsylvania, Maryland, Virginia, and South Carolina,) furnish more than fifty of the most unequivocal instances of the existence of the yellow fever, not one of which has been demonstrated to have arisen from importation, or to have been propagated from a sick to a healthy body. Doctor Mitchell's description of this disease, as it appeared in Virginia, in 1741, is admitted to be one of the

most accurate and complete to be found in the annals of medicine. In 1793, during the gloom that clouded the city of Philadelphia, when the mortality of the disease seemed to defy investigation, it imparted to our illustrious countryman, Dr. Rush, the first spark that lighted into existence the most luminous work that graces the medical annals of our country. Although contagion, at that day, constituted one of the cardinal points of the creed of every physician, and was one of the idolatries of the times, the disease was not imputed to other climes, but ascribed, by Doctor Mitchell, to the tropical season in which it originated.

But why should we dwell on the testimony of our predecessors? Although their works are full of instruction, the experience of our own times speaks a living language more eloquent, and, indeed, irresistible. In every state of the Union has this disease occasionally originated, from local causes, either sporadic or epidemic; even among our ordinary bilious remitting fever we occasionally meet with some of the more inveterate grade.

M. Volney,* during his travels through the interior of our country, found the yellow fever in several places. Having seen it previously in our seaports, he could not have been deceived.

Mr. Ellicott, in his journal, is so explicit and intelligent on this subject, that no reasoning can shake his statement. His words are these: "I arrived at Galliopolis about eleven o'clock in the morning. This village is a few miles below the great Kanhaway, on the west side of the Ohio river,† situated on a high bank; it is inhabited by a number of miserable French families. Many of which, this season, fell victims to the yellow fever. The mortal cases were generally attended with the black vomit. This disease certainly originated in the town, and in all probability, from the filthiness of the inhabitants, added to an unusual quantity of animal and vegetable putrefaction, in a number of small ponds and marshes within the village. The fever could not have been taken from the Atlantic states, as my boat was the first that descended

* Volney's View of the Climate and Soil of the United States.

† Journal of a Voyage down the Ohio, in November, 1796, by Andrew Ellicott.

the river after the fall of the waters in the spring: neither could it have been carried from New-Orleans, as there is no communication at that season of the year, from the latter to the former of those places: moreover, the distance is so great, that a boat would not have time to ascend the river after the disorder appeared that year in New Orleans, before the winter would set in." A more detailed account of the fever at Galliopolis was communicated to me by major Prior, of the United States' army, who witnessed the rise and progress of the disease. The major visited Baltimore in September, 1797; and, at my request, committed to writing what he had seen the year before, in the following words: "As the garrison was severely afflicted by this fever, I could but take great interest in it. The fever was, I think, justly charged to a large pond near the cantonment. An attempt had been made, two or three years before, to fill it up by felling a number of large trees that grew on and near its margin, and by covering the wood thus fallen with earth. This intention had not been fulfilled. In August the weather was extremely hot, and uncommonly dry; the water had evaporated considerably, leaving a great quantity of muddy water, with a thick slimy mixture of putrefying vegetables, which emitted a stench almost intolerable. The inhabitants of the village, principally French, and very poor, as well as filthy in their mode of living, began to suffer first, and died so rapidly that a general consternation seized the whole settlement. The garrison continued healthy for some days, and we began to console ourselves with the hope that we should escape altogether: we were, however, soon undeceived, and the reason of our exemption heretofore was soon discovered. The *wind* had blown the air arising from the pond from the camp; but, as it soon shifted to the reverse point, the soldiers began to sicken: in five days half the garrison was on the sick list, and in ten, half of them were dead. They were generally seized with a chill, followed by head-ache, pains in the back and limbs, red eyes, constant sick stomach, or vomiting; and generally, just before death, with a vomiting of matter like coffee grounds. They were often yellow before, but almost always after death. The sick died generally on the seventh, ninth, and eleventh days, though sometimes on the fifth,

and on the third. As some decisive measures became necessary to save the remainder of the troops, I first thought of changing my quarters, but as the station was in every respect more eligible than any other, and had been made so by much labour and expense, I determined to try the experiment of changing the condition of the pond from which the disease was believed to have arisen. A ditch was accordingly cut; what little water remained was conveyed off, and the whole surface covered with fresh earth. The effects of this scheme were soon obvious. Not a man was seized with the worst form of the fever after the work was finished, and the sick were not a little benefited, for they generally recovered, though slowly, because the fever became a common remittent, or gradually assumed the intermitting form. A few cases of remitting and intermitting fever occurred occasionally, till frost put a period to it in every form. As soon as the contents of the pond were changed by cutting the ditch, the cause (whatever it was) seems to have been rendered incapable of communicating the disease in its worst form."

The following fact, published in the Medical Repository, and communicated by Doctor Watkins, is equally explicit as to the indigenous origin of yellow fever. "At a village called New Design, fifteen miles from the Mississippi, and twenty from St. Louis, containing about forty houses, and two hundred inhabitants, in the summer and autumn of 1797, the yellow fever destroyed fifty-seven of the inhabitants, or more than one-fourth. No person had arrived at this village from any part of the country, where the disease had prevailed, for more than twelve months preceding its appearance." Doctor Watkins resided in the village at the time, and having seen the disease in Philadelphia, testifies to the facts. He also mentions an Indian village that was depopulated by a similar disease, two or three years before.

"In 1799, an irruption of the yellow fever was experienced in Bald Eagle valley, in Mifflin county, Pennsylvania, and was as mortal, in proportion to the population within the sphere of the remote cause, as it ever had been in any of the seaports of our country. This valley is situated two hundred miles north and west from Philadelphia; is deep, and interspersed with frequent

ponds of fresh water, which, from the heat and dryness of the season, emitted a most offensive smell. Near these sources of exhalation the fever prevailed with great malignity. It made its approach with chills, pains of the back, limbs, and head, which, in forty-eight or sixty hours, terminated in death. The sick discharged great quantities of filth from the stomach, of the consistence and appearance of coffee grounds, so offensive as to occasion nausea and even vomiting in the attendants. The feces had the same appearance. In many the disease terminated [fatally] by profuse hæmorrhages."

Fortified by these facts, unsophisticated by the fallacy of theoretic refinement, we appeal to the candour and magnanimity of the advocates of contagion and importation, and challenge them to torture this common sense in physics to suit the visions of their creed. Were these people smitten from the heavens by the malign influence of the stars under which they lived, or was pestilence shaken from the Gorgon abyss of these hotbeds of intestine putrefaction, boiling under an almost vertical sun?

But we are still to enforce our plea, by evidence derived from the demonstration of our own senses, the most unerring sources of experience. In August, 1793, (having been summoned to the eastern shore of this state, in July, in consequence of the death of my lamented father,) the duty of attending the sick, in the sphere of his former practice, devolved on me. After a spring unusually wet, and during a summer as uncommonly hot, the bilious remitting fever became epidemic. On the 9th of August, I was requested to visit Mrs. Hall, consort of Levi Hall, who had been confined three days. I found her in the agonies of death. Her skin of a deep orange colour, her eyes red and prominent, pulse intermitting, and ejecting copiously from her stomach once every eight or ten minutes the secretion, now known by the name of black vomit. She expired in a convulsion while I sat at the bedside. Petechiæ appeared immediately after death, and putrefaction succeeded so rapidly that I advised immediate interment. On the 12th, I was requested so visit William Leighton, manager on the estate of Mr. James Summers, who had been ill of a bilious remitting fever five days. I found him with an intermitting

pulse, a pale yellow complexion, cold hands and feet, red eyes, an incessant hickup, and complaining of a heat in his stomach, which he compared to "burning embers." He died about five hours after my return home, and the scene was closed with the fatal black vomit. On my arrival at twilight, the family were preparing to inter him, and with great difficulty succeeded in getting him into the coffin, as his flesh was literally dropping from his bones. Will the abettors of imported contagion attempt to derive these cases from our seaport towns, when it is notorious that the disease existed in none of the cities except Philadelphia, where it had then recently commenced? Can rational men prevail on themselves to believe that a disease thus originating in Caroline forest, could have been referred to any other source but an inquinated atmosphere? From this date, till the progress of the epidemic was arrested by frost in October, every grade and every variety of miasmatic fever was to be found, and no etiologist, versed in the laws of epidemics, will contend that there existed in the same place, one cause for dysentery, another for intermittents, a third for remittents, and a fourth for yellow fever. During the rainy season, in June and July, the dysenteric form of fever prevailed; but in August it was gradually converted into that of the bilious remittent. Although we might adduce thousands of cases to confirm the domestic origin, and non-contagious nature of yellow fever, we will content ourselves with one more, perhaps more remarkable than the rest. In September, 1799, I was called to visit the family of John Webster, about three miles north of Baltimore, on the old York road. His house stood on a level, apparently beyond the reach of noxious exhalation, as there were supposed to be no stagnant waters within a mile of it. I found Mrs. Webster in bed, under a bilious remitting fever, which had continued eleven days; Webster's daughter, nearly seventeen years of age, suffering from a similar fever; two sons, the one between eight and nine, and the other six, labouring under dysentery; and Webster, on the margin of the grave, from a most malignant fever. I found him without a pulse in his right arm, and one scarcely perceptible in the left, his face and breast resembling a hide of soal leather, his eyes red, his extremities cold, a frequent hickup, attended with dejections

from his stomach of a dark viscid mixture, (as it appeared to me,) of bile and blood. I prescribed for the family; and, on my return, hæmorrhages from the gums and nose had begun, and about two o'clock in the morning he expired, on the seventh day from the attack. As I could not satisfactorily account for the condition of this unfortunate family, while the adjacent country was not unusually afflicted, I determined to investigate the matter as thoroughly as possible. Upon inquiry in the neighbourhood, I found that Webster had occupied the house only about five weeks, and that immediately preceding, a man had died suddenly in it. This led me to a minute examination of the premises. The cause was manifest. The cellar contained water about two feet deep, which had remained there from the first week in June, the country having been then inundated by torrents of rain. The cellar being useless, the door had been closed, and the only vent for the pestiferous gases was through the floor, which was open in several places.* At my solicitation the family was immediately removed, and all convalesced from the time they ceased to breathe the air of the place. But the tragedy did not end there. The owner of the house, anxious to retrieve its character, hired two men, John Wills and Thomas Burke to empty the cellar. They began by ripping up the floor, and having placed a pump in the deepest part of the water, they evacuated the cellar to the dregs in one day. They executed this nauseous task on the 30th of the month; and on the 2d of October, Wills was seized with a chill, succeeded by an ardent fever, which terminated with the usual symptoms of yellow fever, such as hæmorrhages, yellow skin, and petechiæ, and died on the night of the 5th. Burke was attacked the day after Wills, but lived till the 9th, and died with all the symptoms in Wills's case, with the addition of the black vomit. It will not be argued that these victims to a home manufactured poison contracted their death sickness from a sick body, from a secreted virus in Baltimore, because it did not exist there, for the summer and autumn of that year were remarkable for their healthiness.

* On the afternoon of my first visit to Webster's family, I was seized with nausea and general lassitude. I believe a fever was arrested by a strong dose of tartarized antimony, which operated sursum et deorsum.

To every man of accurate observation, possessing the capacity of discrimination, the rise and progress of the three great epidemics of 1794, 1797, and 1800, as they appeared in Baltimore, would alone be sufficient to establish the domestic origin, and non-contagious character of the yellow fever. The disease was exclusively confined to the low grounds covered with the materials of putrefaction, or to those parts composed of factitious materials, corruptible in their nature. In that part of the city west of Jones's falls, the line of demarcation between the pure and deleterious atmosphere, was obvious to the most superficial observer. In 1797, an occurrence demonstrated the locality of the infected atmosphere so clearly, that it wrought conviction in some of the most sceptical of our citizens. Previous to the 17th of September, the fever had been confined to the places already described, and to such as had breathed the air evolved from them: on that day a strong S. E. wind wafted the pestilential effluvia in a N. W. direction, and diffused it among the inhabitants of the upper parts of Frederic, Gay, South, and Calvert-streets, who became immediately implicated in all the horrors inseparable from so malignant a fever. In 1800, several concurrent facts were noted, and the principle became so familiar, that the increment of cases was calculated with tolerable accuracy, by observing the variations of the winds. No case of the yellow fever ever did originate in west Baltimore, above Hanover-street, beyond the sphere of exhalation from the docks, wharves, or made grounds. I defy the whole population of the city to produce a solitary exception; nor can they adduce a single instance, from the hundreds and thousands of those who, after breathing the pestiferous atmosphere below, and sickening there, communicated the disease.

It will be remembered by some gentlemen, who do me the honour of listening to me to-day, that immediately after the embargo was laid in 1807, I publicly invited the attention of the faculty, as well as the police officers of our commercial cities, to the question of the origin of the yellow fever. Although the non-existence of a disease, depending solely upon the casualties of the elements, could not have been considered more than negative proof of its exotic origin, its existence must be conclusive that it was not im-

ported; the intercourse between us and all other nations being then suspended. Notwithstanding this profound calm, when no foreign sail whitened our waters, in August the yellow fever commenced its ravages at Fells' Point, in a spot, it is true, limited, if we compare it with the general population, but co-extensive with the nuisance by which it was generated; and it continued until the vicissitudes of the weather banished it. So perfectly developed were the symptoms of this fever, that they were at once recognized by all who had seen the yellow fever formerly.* Those who were seized with the disease and removed, did not propagate it; nor was any one infected from the atmosphere, but those who had imbibed the poison at its source.

One of the principal deceptions respecting contagion, arises from the fact of the disease having occasionally originated on shipboard. The confined state of the holds of vessels is, unquestionably, well adapted to the generation and concentration of noxious effluvia. Several causes conspire to render such a condition the focus of pestilence. Improper ballast, perishable by putrefaction, as well as cargoes subject to chymical decomposition; and, perhaps, the accumulation of animal excretions about the bodies of the crew in warm latitudes, especially when combined with other sources of putrefaction, may operate as the remote cause of yellow fever.† The narrative of the yellow fever on board the United States' ship General Green, by Dr. Kollock, in June, 1799, furnishes an instructive lesson on this point.‡ The ship was new, and sailed from Newport, Rhode Island, on her first cruise, on the 3d June. As she had been caulked in winter, she became leaky, and thus damaged, more or less, every thing liable to putrefaction on board. The ballast was composed of iron, slate, and clay, in-

* Doctors Allender and Clendinen, who had witnessed the former epidemic yellow fevers of the same place, testify to the character of the fever of 1808.

† As we propose to prove that what is called typhus is not communicated by a vitiated secretion, we hazard nothing in admitting that putrid animal excretions may combine to produce yellow fever. The reasons why this disease is sometimes thus originated, and under other circumstances, a different disease, will be explained hereafter.

‡ Vide Medical Repository, Vol. iv.

terspersed with marine vegetables. The provisions became contaminated, and the fish, which made a part of them, were thrown overboard. The air between decks became so vitiated, that it would not support combustion, a candle would scarcely burn in the cockpit. The disease commenced on the 18th of the month, and its tragic end is well known. The fever did not exist at Newport before the ship sailed, and she touched no where, neither boarded, nor had communication with any vessel till she arrived at Havanna, after the irruption of the disease. It became necessary, in this disabled condition of the crew, to return to the United States; and, on the 27th July, the frigate arrived at Newport. She was subjected to an entire unlading; the sick, with all their clothes, were sent to the hospital. Mark the end of this disaster; neither the citizens, (who could not be entirely excluded from intercourse with the crew,) nor the attendants of the hospital, contracted the disease. Could this have been predicated of any contagious disease?

The Busbridge Indiaman* is a no less conspicuous example, both of the manner in which the yellow fever is engendered, and of its non-contagious nature afterwards.

We will demonstrate, as we proceed, that the ship Hankey, (from which Dr. Chisholm, in his own imagination, scattered contagion over the whole western continent,) originated the disease in the same way. A similar occurrence can at all times take place, on board any vessel, in warm climates, if there be materials liable to putrefaction. If we were to admit that the causes of the yellow fever were introduced among us after being thus manufactured, it could avail our antagonists nothing. *This local infection of the atmosphere is not contagion.* We will admit these gases to be set at liberty on shore. What is the result of our experience, from a fact that has happened in every seaport of our country? Previous to the pantomime exhibitions under our quarantine laws, vessels in this condition were disregarded. In some cases it is certain they infected those who were concerned on board, exposed to their immediate action, but the vis nocens never multiplied, but

* To be noticed hereafter.

did, as it always must of necessity, diminish, being diluted by a purer atmosphere, till it became inert. Those who have put their imaginations on the rack to conjure up contagion, have in all such cases mistaken the person for the place. Although the elements of pestilence were there, concentrated in their most lethiferous degree, they were no sooner unbosomed than they were dissipated into circumambient space, and thus rendered innocent. The annals of the world cannot furnish an instance of the multiplication of the yellow fever, from effluvia proceeding from a vessel. It is essentially necessary, before the disease can become epidemic, that the general atmosphere on shore shall have been changed to the same state. There is no fomes, no fermenting principle in any infected vessel, that can assimilate the atmosphere by the addition of twenty or thirty cubic feet of mephitis. This physical arithmetic by which visionary sophists double, quadruple, and quintuple contagion, exists no where but in their own imaginations. When they speak of "*inoculating the atmosphere*," reason abhors such language, and the genius of philosophy weeps at their folly. The absurdity of this hypothesis will be demonstrated in the sequel.

The manner in which the yellow fever becomes epidemic, is one of the most vehement proofs that it is not derived from contagion. The first effects of the causes, while they are feeble, are frequently manifested only in slight pains of the head, stomach, back and extremities, and other indications of the operation of the remote cause, all of which are the unerring preludes to more inveterate grades. If the disease were derived from contagion, the more pestilent features would develop themselves at first, as certainly as at any subsequent period; for it is an immutable law of all contagions to maintain nearly an uniform tenor of action, as long as they exist.

The dispersed state of the first cases, renders it at once physically impossible, that the disease should have been propagated by contagion. The pestilent rays do not diverge from a centre to a circumference, but commence at the same time at every point among those who hold no other intercourse than the common atmosphere they breathe. In all instances of contagious diseases,

the increment of cases is in the ratio of the bodies to which the contagion attaches, and its track is so distinctly marked, that it can be retraced to the first case. The yellow fever has no focus, and, therefore, no pestilent rays are reflected. If this disease were derived from a vitiated secretion, each succeeding case would be as capable of multiplying itself, as that from which it first originated. Contagion, in its true sense, does not lose an atom of its essence during its existence, but unceasingly generates its own similitude, as long as there are bodies to assimilate to its own nature.

The extinction of the yellow fever by frost, shows that the cause exists in the atmosphere, for all contagious fevers are more so in a low temperature. So true is this principle, that many of them cannot exist in tropical climates. On whatsoever essence they may depend, it is volatilized by heat, and thus rendered innoxious. Even the small pox is less contagious in warm weather. The yellow fever is the very antipode of this, heat is the sine qua non of its existence; in heat it originates, by heat it is supported; deprive it of heat, and its remote cause is instantaneously annihilated. If a vitiated secretion propagate the disease, and is essential to its communication, it necessarily follows, that in autumn, when our houses are shut, and all matters to which contagion can adhere, or has an attraction, surround us, we are destined to disease. It would be a paradox too absurd for human credulity to believe, that a gazeous emanation from a sick body could cleave to the sails, the masts, and spars of a ship, or insinuate itself into the clothes of a seaman in Africa, Asia, or even the West Indies, retain its pristine virulence, and inflict disease and death on our citizens the moment a vessel shall have anchored in our ports. How stands this chimera when opposed to the universal experience of mankind, which teaches us that it cannot be propagated at the distance of two feet in our hospitals? Surely there must be some nondescript Bahan Upas, some Tartarean quintessence, sui generis, hitherto unknown, that can thus flash perdition from the equator to the poles!

The convertibility of the yellow fever into the milder forms of miasmatic disease, argues a common origin in the cause. Almost

every practitioner of experience in the malignant bilious remittent of our country, bears witness to this fact. By the proper antiphlogistic treatment, an inveterate yellow fever has been reduced to an ordinary remittent, and sometimes to an intermittent; just as a remittent, in ordinary seasons, is made to assume the form of an intermittent. In the summer of 1797, at Baltimore, the symptoms of dysentery were gradually commuted for those of a yellow fever of the deepest die. From the middle of June till the last of July, a time of intense heat and reiterated torrents of rain, the *dysenteric* form of fever pervaded Fells' Point. In August, the rains having ceased, and the weather continuing unusually hot, the dysenteric symptoms failed, till they were lost in the unequivocal characters of the yellow fever. At first a nausea, a red or watery eye, with pains of the back and limbs, all in an inconsiderable degree, accompanied a severe tenesmus with frequent dejections of mucus and blood, without a yellow skin; but as a less humid atmosphere succeeded, the intestinal affection declined, and gradually vanished, till it was obliterated. A fever of a more inflammatory type, with an acute headach, frequent evacuations from the stomach, a burning heat, an obstinate constipation, and a yellow skin, composed the train of succession.* This transmutation is incompatible with the laws of contagion. There cannot exist two general fevers in the same body, at the same time, and the phenomena are to be accounted for only on the principle of a variation in the remote cause resident in the atmosphere. No adventitious cause of fever can modify the features of the case, if contagion shall have first invaded the nerves. It carries its peculiar character immutably stamped upon it, and marks its sanguinary track wherever it is to be found.

The negative of the controversy may be established on another ground, which defies all the machinations, and sets at nought all the visions of theoretic intoxication. It is admitted by all, that the subjects of certain countries are unsusceptible of the yellow fever in our own. In the most malignant and protracted epidemics that have affected us, the remote cause has proved inert to them.

* Doctor Coulter is a living witness to testify to this fact.

The emigrants from the West Indies, and other warm latitudes, especially the native French, have invariably resisted the force of the cause, until a residence of some years shall have assimilated their constitution to the American habit. The immunity from the operation of the morbific cause, is explicable solely on the principle of atmospheric infection. The habitual impression of heat, the constant evolution of miasmata, in their native countries, had rendered them insensible to the exhalations in this. Having resided among us a few years, after the cold of our winters had broken the force of habit, and permitted an accumulation of their excitability, they are rendered as sensitive, and are as exquisitely impressed by the cause, as our own citizens.*

If we reverse this position, and observe the influence of an inquinated atmosphere upon those who have inhabited more northern climes, our position is still supported by general experience. Those who may have recently arrived, are susceptible of the remote cause in the exact proportion to the latitudes whence they came. Those from the north of Europe scarcely ever escape an attack. The subjects of the United Kingdoms of Great Britain are little less sensible to the deleterious cause; and even those from the more northern of the United States, are more liable to the disease than the citizens of the southern and middle states. In 1797, 1800, and 1802, the New Englandmen were always the first and severest sufferers, of our own countrymen, provided they had not spent a summer or two in our city. It follows of necessity, that all such persons are doomed to suffer a more malignant and dangerous grade of fever. If the disease had been communicated by contagion, this attraction for one people, in preference to another, would be inexplicable by any known mode of reasoning. There is no contagion that was ever known to spare any race of men that came within the sphere of its action. It matters not, whether the subject be born in the east, in the west, in the north, or the south; whether he be a Christian, Jew, or Musselman, once the poison shall have smitten his nerves, his destiny is fixed.†

* Making the proper deduction for the abstemious mode of life adopted so generally by Frenchmen.

† I speak of a general rule, which must admit of occasional exceptions. There is one in some thousands who will not receive the small-pox.

It has been asserted, that the yellow fever of the West Indies has prevailed only, or principally, during war, which necessarily brings together a greater concourse of men; and that contagion is thus engendered from a *fomes*, a word synonymous with a vitiated secretion, if we are permitted to attach to it any signification in medical parlance. This opinion, imposing as it may appear at first view, is true only in one sense. It is true, that greater numbers have received the disease, in a given time, during war; but this fact militates against the hypothesis it was intended to support: it proves that in time of war a greater mass of men, not inured to a warm climate, were compelled to breathe an atmosphere that teemed with the remote cause of the disease. If we refer to the histories of all the wars, of which the West India islands have been the theatre, either wholly or partially, we will find that the yellow fever has not appeared as often during war as in the times of peace. Calculate the periods of peace and war, from the first European settlements to this day, and the history of every island in the West Indies conspires to prove, that two-thirds of the epidemic yellow fevers that have infested them, have prevailed in times of profound peace.

But Doctor Chisholm has declared the yellow fever contagious, and as that of the West Indies (which has prevailed time immemorial) has been, by common consent, declared not contagious, he has manufactured *a new one at Boulam*, and we have now an African and a West India yellow fever. Many of the former advocates of contagion had abandoned the opinion as untenable, but since the epidemic at Grenada, in 1793, they have (with a few exceptions) rallied under the banners of Doctor Chisholm, and he is now the luminary around whom all the satellites of contagion revolve. We will test the merits of his doctrine by his own writing. Doctor Chisholm admits that the yellow fever of the West Indies is not contagious; but contends that a contagious fever was generated at Boulam, transported to Grenada by the ship Hankey, and thus propagated to the inhabitants. From the cotemporaneous existence of the yellow fever at Philadelphia, he and his adherents have deduced the conclusion, that it was conveyed from Grenada to Philadelphia. This opinion is corroborated by no

facts, but rests on the baseless ground of hypothesis. The character of the fever of Grenada, drawn by Doctor Chisholm himself, corresponds, in all essential points, with that which has always prevailed in the other islands. The yellow fever of the West Indies is now as it was described by Doctor Warren and Doctor Desportes, and always has been since the settlement of the islands by Europeans. The disease has not been more severely nor generally felt, since the pretended importation from Boulam, than it was before. The concurrent testimony of the physicians residing in the islands has established this fact. How did the yellow fever that infested New York in 1791 originate?* Why was it called the dock fever? It would be as rational for Doctor Chisholm to maintain that it was *exported* thence to Boulam. A brief review of the Doctor's account of the Hankey's voyage, will place this question in a more perspicuous light. "Boulam," says the Doctor, "being surrounded by the sea, enjoys all the advantages of a free ventilation; and being dry, and not incommoded by any marshy tract, is considered the healthiest spot on the windward coast. It is not inhabited, but occasionally visited by the inhabitants of the neighbouring continent." The island was not inhabited. How then did the Doctor learn that such a disease as the Boulam fever existed in a desolate island? If it were received among his crew by contagion, the vitiated offspring of secretion, it must have been derived from some human being. "This fever," adds the Doctor, "was communicated exclusively by contagion!" The Doctor proceeds to state that the negroes, on this part of the African coast, are ferocious in an extraordinary degree, and even reputed to be *cannibals!* Eaters of human flesh, prowling for man, whom they seized only to devour. It would be absurd to suppose that the crew of the Hankey cultivated an acquaintance with them. The Doctor continues the same style of complaint, and remarks, "from this circumstance, we could erect no accommodation on shore, during the nine months the Hankey laid there, and the intended settlers were obliged to live on board,

* Vide Addoms's Inaugural Dissertation on the malignant fever that prevailed in New York in 1791.

while the rainy season coming on almost immediately after their arrival, the heat at the same time being excessive, they endeavoured to shelter themselves from both, by raising the sides of the ship several feet, and covering her with a wooden roof."* In the next sentence the Doctor (unintentionally) reveals the real source of his disaster: "The confinement of upwards of two hundred persons in the narrow limits of a single ship, in an excessively hot and wet atmosphere, the want of cleanliness, and a depressed state of mind." From this expression, it cannot be doubted that the fever was engendered on board. If Doctor Chisholm had determined to denominate the disease from its place of origin, he should have called it the *Hankey fever*. Who, however superficially acquainted with the effects of putrefaction, in warm latitudes, can doubt the cause of this disaster? Would not an enlightened etiologist be astonished if no such consequence had followed?† The case of the Hankey is not a solitary instance. "The Busbridge Indiaman sailed from England for Madras and Bengal, on the 15th of April, 1792: on the 26th of May she crossed the equator, in 27 degrees west longitude. The mercury ranged from 80 to 86 degrees; the weather was very sultry, with frequent rains. In this state of things the yellow fever broke out," says Doctor Brice, "although we had touched at no port, nor had communication with any vessel."‡

Livy§ mentions a disease in the Roman army, which he denominates by the general term pestilence, and expressly declares that it arose "*Ex colluvie omnium generum animantium in castris.*"

As we admit that in these circumstances the product of putrefaction must have consisted in part, at least, of animal excretions, the favourers of contagion will, no doubt, eagerly embrace the fact, to form an alliance between the yellow fever and typhus; and, perhaps, attempt thence to deduce the identity of their essence.

* Vide Chisholm on the Fever of Grenada.

† Nor was the fever communicated from the Hankey to the inhabitants of the island of Grenada. It has been incontestably established that the disease appeared in that island before the ship arrived. [*Medical Repository, Vol. I.*

‡ Brice on the Yellow Fever.

§ Book IV.

We admit also that animal excretions can act as the cause of fever by becoming putrid *after they are excreted*, in common with the result of vegetable putrefaction. But this does not prove a vitiated secretion, a morbid action of the arteries, by which only contagion can be formed. On ship-board it is always physically impossible that the materials, subject to decomposition by putrefaction, can be either wholly animal or vegetable, and hence we account for the well known fact, that in all fevers on ship-board, in camps, jails, and all other places where a crowd of men are immured in a small compass, we find the character of the disease to depend upon the proportions of animal and vegetable matter concerned in its production, and on the degree of heat. If the occurrence take place in winter, or in any temperature below 70 degrees of Fahrenheit, a fever of a low continued type, called a *typhus*, will be the inevitable result. If the incident happen in a higher temperature, as in the cases of the Hankey and Busbridge, a disease compounded of the symptoms of the tropical endemic and typhus will be the result. If we remove seamen from a ship, and place them on shore, in an atmosphere pregnant with the remote cause of the endemic yellow, or even the milder grades of bilious remitting fever, each succeeding case will more and more assume the aspect of the disease of the place, until the predisposition to typhus shall have been completely lost. Reverse the position, and transfer the landsman thus predisposed, to a ship in the harbour, the crew of which are infected with typhus, and he will be attacked by a disease at first complicated, and every subsequent case will wear a deeper typhus complexion. It has been laid down as an axiom by some, that the causes of these diseases bear no affinity to each other, and that nature has drawn an inviolable line between them; on this point we judge from facts alone, and from repeated observations we judge with sufficient precision. We have witnessed three examples of the mutual convertibility of these predispositions by change of place, and facts abound in medical writings which conspire to evince the same conclusion.* Although human miasmata and vegetable effluvia are

* Vide Lampriere on the Diseases of Jamaica, vol. ii, page 82; to which we may add, that marsh effluvia, under any circumstances, must of necessity

equally the result of putrefaction, their action upon the living system appears to be different. Human effluvia would seem to inflict a more poisonous and stupefactive wound upon the nerves, impairing their mobility, and rendering them less capable of high re-action; while that arising from vegetable decomposition would appear, not only less deadly to the organs of sense, but calculated to render them more exquisitely sensible. The operation of the former would seem to justify the conclusion that it does not alone occasion the least disarrangement in the functions of the hepatic system, while the latter invariably molests the operations of the liver. In what this difference essentially consists, we cannot determine in the present state of chymical science. No process has yet been instituted by which we can analyze these elements so far as to mark a definite distinction in the effects of their action on living matter. It is sufficiently manifest, that heat disposes the animal cause to become less noxious, and often inert, while it imparts life and energy to the product of vegetable putrefaction.

The subject may be still contemplated in a different, and perhaps, a clearer light, by comparing the yellow fever with diseases really contagious: but before we can proceed intelligibly, it behoves us to adopt some definite sense of the word contagion. We are to inquire whether the words *contagion* and *infection* are synonymous and convertible terms, or whether they are as different in a philosophical, as they are in a philological sense. This will be difficult, if not impracticable, unless we revert to their primitive significations. It has been insinuated by some, that contagion is a term of modern invention. This error may be easily refuted by a reference to ancient authorities. Although many late, and all ancient lexicographers use the words contagion and infection as synonymous, in a medical sense, there can be no doubt but they were originally intended, in common parlance, to convey different meanings. It would appear that even the most illiterate among the ancients spoke more correctly than the literati of the present day. The terms *lues pestis*, *contagio* or *contagium*, and *infectio*, will be considered in order.

partake, more or less, of an animal nature. Among vegetables there always exist myriads of insects, which undergo the putrefactive process in common

The word *lues* is probably the most ancient generic term used to express the fact of a disease being communicated from a sick to a healthy body. It was primarily confined to such diseases as were supposed to entail a penalty upon those who contracted them, or to pay a forfeit for some criminal act. Hence the term attaches, with propriety, to *lues venerea*, a disease propagated by the illicit commerce of the sexes. We will show, that although it was first intended to signify a disease communicated by *actual contact*, it was also employed in a looser sense, and at a very early period; of this there are many examples in ancient writers, and unfortunately the true sense has been departed from, and *lues* transferred to those great pestilential visitations, whose causes were even then referred to the atmosphere. Claudian says,

"Hinc hominum, pecudumque lues, hinc pestifer aer."

Virgil uses the word *lues* to express the opinion that the cause of a deleterious influence, on vegetable life, resided in the atmosphere:

"Arboribusque satisque lues, et lethifer annus."

The word *pestis*, although now applied in medical phraseology to a particular disease called the *plague*, appears originally to have been applied to any widely extended desolating epidemic, from whatsoever cause it might have arisen. The most ancient records of the prevalence of pestilence convey the idea that they were inflicted by the hand of the Almighty, as a punishment for some signal national iniquity. When Pharaoh refused to liberate the children of Israel, and his heart was so hardened as to hold them in bondage, pestilence constituted one of the seven plagues of Egypt.—Virgil has ascribed the origin of pestilence to the anger of the heathen gods. "*Pestis est ira Deorum Stygiis sese extulit undis.*" The το θειον of Hippocrates corresponds with the superstitious belief of the ancients, that pestilence was always inflicted by divine authority. It is evident that Virgil's language alludes to an endemic origin; the words *stygiis undis* admit of no other in-

terpretation. It cannot be doubted that the word contagion is a compound of the words *con*, and *tango*, taken in the most literal sense. As Doctor Quackenboss* has justly remarked, the term applies particularly to gregarious animals, such as the human species, sheep, and cattle. The social nature, and constant intercourse of these species, is such, that the actual contact of their bodies must, necessarily, be a frequent occurrence. Martial, in one of his epigrams, speaks thus:

"Depascentur oves diri contagia morbi."

Virgil uses the word *contagium* in a similar sense, when Mellibæus assures Tytyrus that the noxious contagion of the neighbouring flock shall not injure his :

"Nec mala vicini pecoris contagia lædent."

If the mischief, ascertained by melancholy experience to have arisen from this intimate association, be not a secreted virus, rendered poisonous by the very act of secretion, it must have been an accumulation of matter chymically decomposed in the process of putrefaction. It is sufficient for us to prove that a disease was thus propagated. It would seem fair to conclude, that the ancients received the words *contagio*, *contagium*, and *contages*, in the first sense, before the other, more indefinite, was adopted. We define contagion, *a vitiated secretion, brought into contact with the nerves by inoculation; or a morbid secretion, emanating from a sick to a healthy body, and received into the lungs, giving rise, without the agency of any other cause*, to its own similitude in *a body previously healthy*. We propose to illustrate this definition by examples; but we will first define infection.

The word *inficio*, compounded of *in* and *facio*, signifies, literally, to undo; but it is also used by the best writers to imbue, as with a dye, to stain, to tint, violate, corrupt, or spoil. Whatever made a material change in a substance to which a foreign matter was applied, either in colour or texture, was said to *infect* it. For

* Quackenboss on Dysentery.

these reasons, when the miasma, arising from putrefying substances was exhaled and mixed with the air, the atmosphere was said to be infected. The air we breathe having thus received a foreign mixture, is rendered unfit for healthy respiration, and is said to be infected. The atmosphere thus perverted from its healthy destination, and received into the lungs, *infects* the body before healthy. Whatsoever was infused into that which was previously salubrious, is said to *infect* it, or so to alter or change its qualities, as to unfit it for any salutary purpose. Hence the reflection cast, by the poet, on unnatural stepmothers:

"Pocula si quando sævæ inficere novercæ."

In these senses, derived from the Latin, the word *infection* has been employed, and received, in a popular acceptation, from the invention of the English language to this day; but was not appropriated as a medical discriminative appellation till the distinction between a vitiated secretion, the result of living vascular action, and that of putrefaction in dead matter, was suggested to modern etiologists, in attempting to explain the causes of epidemics. From this obvious difference, in the sources of the two great causes, which originate the greater part, and in the opinion of some, all epidemic fevers, it becomes necessary to draw a line of demarcation between them. It would be difficult, if not impossible, to invent words that would distinctly express the necessary discriminations. Those *infecting* matters, the consequence of animal or vegetable decomposition, might be aptly called the *sarcoseptick* and *phytoseptick* products, and would correspond with our views of the origin of typhus and all the tribe of intermitting and remittent fevers, from the mildest ague to the most inveterate yellow fever. But there is still a desideratum, something to mark contagion in its true character, the change wrought by the very process of morbid arterial action. As every poison, thus assimilating to itself a part of the living matter on which it acts, exerts a peculiar action, we call it specific, because it is succeeded by a train of symptoms, sui generis. It follows, from this notorious fact, that a name, discriminating each of them, cannot be appro-

priated with any certainty, because we cannot, by any known mode of philosophizing, demonstrate their respective essences. If the chymic art should ever succeed in detecting their elements, and rendering them cognizable by its laws, we may then invent terms sufficiently expressive of all their qualities. Notwithstanding this deficiency of conception of the essential qualities of secreted animal poisons, we might perhaps contribute comething to a clearer understanding of the modus agendi, of the different causes inducing contagious diseases, by designating such as are the result of morbid vascular action, by the generic term *toxicopoetick*, or poison engendering agents. As it is well ascertained, that in fevers proceeding from putrefaction, none of the secretions are capable of re-producing their own similitude, and that those fabricated by the action of diseased vessels always can, and generally do, multiply in the ratio of the number of surrounding healthy bodies, some such distinction is indispensable.

If we search the writings of the ancients we will find, that although the popular sentiment was the literal contact of parts, when they spoke of contagion, the physicians recognized no distinction between *person* and *place*, when they treated of fevers. If we except the writings of the Arabian doctors, Rhazes, Avicenna, and Avazores, who treated expressly of the small-pox and measles, we find no well defined account of the character of contagious diseases. Galen is the first physician who intimated that other fevers were contagious. He speaks of ophthalmia as a contagious disease, and acknowledges he fled from Rome, and returned to Pergamus, through fear of the contagion of the plague; but no where takes any distinction between the place in which the disease prevailed, and the bodies of those who had it.*

Aretæus, in his Therapeutics, asserts, without reserve, that the elephantiasis is as contagious as the plague.

Aristotle mentions† the plague to be contagious, but, like Galen, Aretæus, and many others, does not define it better than the favourers of the opinion at this day.

* Lib. 1. C. 2. De differentia febrium.

† Prellem. 7. Sect. 1.

Notwithstanding the epithet *contagious* had been employed time immemorial, to signify almost every fever which could be contracted by being in the place where it prevailed, it never was applied, in a personal sense, to any general disease except the plague and the eruptive fevers, till it was suggested by Hieronimus Fracastorius; and no man of a sound discriminating judgment will decide, that his opinion was conscientiously expressed. Every circumstance connected with the case evinces a dereliction of principle in the author, the consequences of which are as deplorable as its propagation has been general. This futile hypothesis was the offspring of a political incident which originated in the conflicting interests of the rival potentates, Francis I. of France, and Philip V. of Germany. During the session of the memorable council of Trent, in 1547, a spirit of dissention arose among the members of that august body. The question whether the council should continue its session at Trent, or remove to the city of Bologna, was warmly agitated; and, for some time, the scale appeared to preponderate in favour of the former; but the interest of Pope Paul III. finally prevailed, and the council was accordingly translated to the latter.* While the issue of the controversy was doubtful, and the pope considered the completion of his views as problematical, among the various other means employed to ensure success, he found, and enlisted in his service, a zealous and ingenious advocate in Hieronimus Fracastorius, a popular physician at Bologna. It happened that in the most critical period of the contest, a fever appeared at Trent, which, although it occasioned little or no alarm in others, the occurrence was eagerly embraced by the doctor, and wielded as a most powerful engine against his antagonists, and in favour of the pope's interest. This disease, which would seem from his own account to have been a common *typhoid* fever, was painted in the most terrific colours his imagination could invent, and made the basis of a work which represented it, as *contagious* as the small-pox, one which none could

* The decree for the removal of the council from Trent was passed on the 11th day of March, 1547, entitled,

"De translatione Concilii sacrosancti et Œcumenici Tridentini Decretum die XI Mensis Martii MDXLVII." Acta Concilii, page 69.

approach with impunity. He represented it as having a peculiar attraction for noble blood, and thus struck a panic through the council, overwhelmed the emperor's delegates and the Spanish bishops, and thus the pope's party triumphantly sat in council in his own city of Bologna. This stratagem, though evidently a *ruse de guerre*, is a remarkable example of the incalculable consequences that sometimes flow from the most trivial incidents. The delegates to this illustrious council were no less dignified as philosophers than distinguished as theologians; they were powerful as men, independent of that influence with which their sacred characters clothed them; but they were not *medical* philosophers. They returned home deeply infected with this *fatal* error, which has slain its thousands and tens of thousands, and thus perpetuated it throughout Europe. Previous to the dissemination of this hypothesis, of febrile contagion from this prolific source, the nations of Europe knew no fears from the imaginary introduction of epidemic diseases, except the plague and the small-pox, but it follows as a necessary consequence to the propagation of such a sentiment, that these commercial impositions, under the several forms of *quarantine*, *pest-houses*, *lazarettoes*, *and even banishment*, followed in the melancholy train of accumulated aggravation, and still continue, to the disgrace of civilized man, and often to the infinite embarrassment, and sometimes the ruin of the enterprising and industrious merchant.

If we apply the doctrines we have attempted to inculcate to some other diseases, we may arrive at the same conclusions by a different mode of ratiocination. As the laws imposed on contagious, and non-contagious diseases, present phenomena at variance with each other, a comparison between them, with some experiments and just deductions from both, may present the subject in a different attitude. We will proceed by a mode as exemplary as the nature of the subject will admit of.

The *lues venerea* and *psora*, are appropriate examples of contagion in its pristine, literal sense, because they are not communicable, unless by actual contact, and in a palpable form. No effluvium from either of these secreted poisons can re-produce them. Applied to the cuticle, they both erode it, and are thus communicated

by inoculation, in the proper sense of the word. The small-pox belongs to another description of contagions, and differs from the examples cited. Notwithstanding it is a general disease, excites high phlogistic diathesis, and often exerts a deleterious influence on the system, the virus secreted will not erode the cuticle,* and it becomes necessary to remove it before inoculation can be effected. It can, nevertheless, re-produce itself by an effluvium emanating from the diseased body, when received into the lungs, although it is so subtile as to evade the keenest optics. All the local contagious diseases belong to the first, and the general to the latter class. The local are capable of being received more than once, the general contagious fevers but once. These two axioms are laws of nature, as universal, as unerring and inviolable as the laws of attaction or gravitation. There is not, never has been, nor ever will be, a general contagious fever, that can be more than once tolerated in the same human body. Every contagious disease could be multiplied by inoculation, if we could devise means of concentrating its peculiar essence, and rendering it so gross as to become subject to the senses of touch and sight.

There is another variety of fevers reputed contagious that obeys laws different in some respects. The *rubeola* and *scarlatina*, seem to bear some affinity in their laws to variolæ, both from their eruptive character, and from the fact that they generally render the system unsusceptible of the impression they make for ever afterwards. Both these fevers are admitted to hold their primary causes in the atmosphere, and are as generally admitted to be contagious; nevertheless there is something exceedingly equivocal in their generally reputed contagious properties. There is always much difficulty in determining absolutely whether a disease be contagious or not, when the cause of that disease is diffused through the air we breathe. That both these diseases are less contagious than small pox, is a fact well ascertained, and it is the general opinion, that scarlatina is less prone to multiply itself than

* When Lady Mary W. Montague introduced inoculation into Great Britain, she thought the Turks succeeded in communicating the small-pox by the simple imposition of variolous matter upon the cuticle, but this opinion is justly exploded. (*Vide Lady Montague's Letters.*)

measles. The peculiar fetor expired from the lungs in those diseases, although not precisely the same in each, is analogical proof that they are both imparted from that source, although it may be questioned whether the fetor, in scarlatina, proceed from the lungs or the fauces. It has been a received opinion that the measles and scarlatina can be propagated by immediate contact with the nerves, as in the small pox, though with less certainty. The opinion of Dr. Home would seem to favour this opinion, as it respects the measles, but we must deduct something from the validity of the Doctor's conclusions, when we reflect that his experiments were performed in an atmosphere infected with the cause. We also have, on two occasions, instituted a similar series of experiments, with the cuticle, tears and blood of morbillous patients, but never could propagate the disease in a given time, correspondent to that law which is imposed upon every disease derived from inoculation, and therefore, incline to the opinion that the diseases which followed were derived from the general constitution of the air. The measles observe almost uniformly other laws, at variance with small pox, and other contagions. They appear periodically, nearly once in five years, generally early in January, in our country and in Europe, and as uniformly disappear about the beginning of summer. All contagious general diseases, (except the cynanche parotidæa,) are less disposed to multiply in warm weather. Solitary cases of measles have often been introduced from the contaminated atmosphere of a neighbouring state into this city, but on no such occasion have they become epidemic, nor have they been thus communicated to any individual within the sphere of my observation. It would appear from these facts, that a general diffusion of the cause through the atmosphere is essential to their multiplication.*

The *pertussis* observes laws materially different from any other disease supposed to depend upon a vitiated secretion. The cause, although obviously resident in the air, is inscrutable, and we can demonstrate its existence only by its effects on the lungs. It sel-

* In September, 1814, the Pennsylvania militia introduced the measles into this city, but the disease was not propagated among the Maryland and Virginia troops.

dom alone excites high inflammatory diathesis, but by its action on the organs of respiration, probably causes a virus to be secreted, volatile in the extreme; and, although not cognizable by any of the senses, holds a singular elective attraction for the system of vessels in which it was generated. It adheres to the seat of action longer than the vitiating principle of any other fever, but is inert as to the lungs of all who have experienced its action, for ever after. It invades the system at all seasons, but does not commence its progress as an epidemic, indifferently, at all seasons, like the mumps.

The *cynanche parotidæa* is an affection of the parotid glands, arising from a peculiar vascular poison, occasioned by the morbid action of these glands, from a similar poison. This disease arises alike at all temperatures of the atmosphere, and no degree of heat, or the want of it, has the least influence on its contagious character. The virus secreted and poured through the parotid duct into the mouth, (like some others,) is so ethereal that it cannot propagate itself, unless during the near approach of those who never suffered its action. It is necessary that the effluvium should be received into the mouth, before it can re-produce itself, and thence the conclusion has been drawn by some, that the *mumps* are not contagious.

There are other fevers contagious, in the estimation of many eminent men, such as cynanche maligna, and ophthalmia. The former is no more entitled to the epithet contagious, than the other species of the genus cynanche. It is a typhus grade of fever, with anginose symptoms. The reflections we propose to offer on typhus, will be an ample refutation of the sophism, that there is any vitiated secretion in that disease which is known by the unscientific appellation, putrid sore throat. We might, with as much propriety, affirm that the putrid alvine excretions, in a bilious fever, were capable of communicating contagion, as the serous excretions from an ulcer of the throat. We find hundreds of examples to prove, that only a solitary individual, of a numerous family, has experienced an attack of this disease. With respect to the latter, it would be superfluous to assert that we have no such disease in our country as a contagious ophthalmia. It has been declared, by high authority, that the ophthalmia of Egypt is contagious, and

that it was communicated from this source to the British troops, during the expedition to that country, in the late war. It would rather appear, from the best digested tracts, that the disease was derived from the influence of a climate operating on men never before exposed to it.

The plague is the only remaining fever to be considered. I know nothing of it except from books. Although I have no hesitation in expressing my *opinion*, that it is not contagious, I will not affirm the negative of this question. I will not imitate the Army Medical Board of Great Britain, by deciding imperatively respecting the character of a disease I never saw. Few, if any, of that learned body ever saw the *yellow fever*, and can know it but imperfectly, only by tradition. They have pre-judged the question, and stigmatized us as illiterate and inexperienced, who have served campaign after campaign in the field of observation and experience. As well might the raw recruit, who "never set a squadron in the field, or the division of a battle knew," compose memoirs on military tactics.

But our antagonists maintain the position that the yellow fever is a species of typhus. To meet them on their own ground, we will prove that typhus has no better pretensions to the character of a contagious disease than any other, the result of putrefaction.

As this disease is reputed contagious by almost every physician of Europe, as well as by a majority of the faculty in the United States, we anticipate the indulgence of those to whom the subject is familiar. The term typhus was used by Hippocrates, and probably invented by him. It is from the word Τυφος, fumus, and this substantive is derived from the verb ΤΥΦΩ, *inflammo, fumum excito.* From these derivations it is probable that the learned father of physic did not intend to restrict the meaning of the word to diseases of feeble vascular action, and that the moderns have made an unfortunate attempt at refinement, by innovating on his meaning. He did not employ the word to designate a genus or species of fever, but used it to mark a particular condition incident to many fevers; and we find accordingly, in various parts of his writings, that in all fevers accompanied with a *stupor*, or *coma*, he introduced the substantive in a metaphorical sense, because it ad-

mits of the translation in an adjective signification, when applied to fever; *smoky*, *thick*, *cloudy*, or *stupid*. From this origin the term has acquired a despotic influence in our nosologies, and is now *forced* to designate a class of fevers that constitute the most formidable phalanx in the long and black catalogue of diseases. Notwithstanding the nosologists have stamped all fevers of low vascular action with the name of typhus, there are several manifestly diversified in their characters, which have received this appellation. The disease known by the name of the cold plague, in Europe, is described by authors as a typhus fever, on account of the weak vascular action that constitutes its principal feature. Of this precise character is the fever that has prevailed of late years in the eastern and middle states of our country. It is reduced almost to demonstration, that a humid atmosphere, and a deficiency of heat, constituted the remote cause of this disease. It has not pervaded the more southern section of our country, and is unknown in all warm latitudes. In jails, hospitals, camps, and on ship-board, in cold latitudes, what is called typhus, sometimes arises from the same causes, but is often modified by the accumulation of animal excretions necessarily inseparable from such places, and the place has given a name, though one not altogether characteristic of the disease. It has been described by medical writers, under the several denominations, *jail*, *hospital*, *camp*, *ship*, *putrid*, and *petechial* fever, with some others, no less descriptive of the real cause. The condition of the air in such situations, and the effluvium emanating from bodies long confined in them, have often imparted the cause of fever to those unaccustomed to them, even although none of the persons confined indicated a symptom of the disease; and whether the fever existed there, or not, it has always been imputed to *contagion*, a peculiar secretion, the exclusive manufacture of febrile action.* This se-

* Great numbers of men brought suddenly together from different quarters, especially in armies, often generate typhus, although none of them may have been previously sick. When the New England troops were first incorporated with the Virginia and Maryland new levies, at Brunswick, New Jersey, in 1778, the camp fever arose and spread rapidly among the latter.

cretion, supposed to be peculiar, and imagined to be as pernicious as that of small-pox, is thought to be generated in the extreme vessels, and either deposited on the surface of the body, or to arise from it, in a form so subtile as to evade the senses. The cause is not cognizable by the senses, but that it is not a secreted poison, is demonstrable by the notorious fact, that no matter on the skin will reproduce the disease in a healthy subject. We have so often performed this experiment with the secretions of the skin, under all the most aggravated states of typhus, that we defy all remonstrance against the facts. Such experiments, with similar results, cannot be predicated of any contagion. Every one that has yet been discovered obeys the uniform law: when introduced into a part, organized similarly to that whence it came, it never fails to generate its own similitude. The whole scale of animal nature cannot produce a single exception. We arrogate nothing to ourselves, when we assert that facts evince the impossibility of the disease being propagated by contagion, and that an abundance of them have occurred within our own observation. We can but persuade ourselves that some of them will render further theoretic reasoning a work of supererogation. In June, 1798, I was appointed, by the secretary of war, to take charge of a corps of United States' artillerists in this city. They were quartered, during the summer and autumn, in spacious well ventilated barracks, in the western precincts; but as these quarters could be held no longer, on the first of November they were removed to the city. It was expected the corps would be transferred to Norfolk in a few days, and an old wooden building was rented for the temporary reception of the men, which was not large enough to accommodate half the number that were crowded into it. The commanding officer, major Bruff, was at Norfolk, lieutenant Muhlenburgh was absent, and the junior lieutenant, Heath, was sick. In this state of things, a general relaxation of discipline ensued, under the command of an orderly sergeant, whose orders were seldom obeyed. Thus crowded together, in weather unusually cold, and uncommonly wet, any amelioration of the condition of the men was impracticable. The house could not be scoured, nor could its contents be washed for want of dry

weather. Thus situated, and detained much longer than had been anticipated, on the nineteenth day after their removal, three men were seized with typhus. It commenced among those who were removed from the former quarters, and it invariably happened that the newer the recruit, the greater length of time elapsed before he became infected. On the 18th of December the major arrived in Baltimore, and found 120, out of 166, on the sick list. I immediately explained my views, and a general purification was ordered and promptly executed. The floors and walls were scoured with *hot lie*, and every article of clothing, except hats and boots, was scoured with soap, and dried in the open air. The soldiers remained in the same quarters till after the beginning of the new year, and recruits were admitted almost every day, but obedience to orders was strictly enforced. The quarters were improved in no other respect, and no other change was made but the simple act of removing the matters accumulated on the bodies and clothes of the troops. This general purification was performed on the 21st and 22d of December, and not a man was attacked afterwards. The sick convalesced rapidly, although, previously to the purification, no means availed in removing the disease. The deleterious influence of the atmosphere of the house fastened upon the brain and nerves a deadly poison, that resisted the whole routine of stimuli, both durable and diffusible, and evacuations were seldom admissible.*

We appeal to the reason and candour of the most enthusiastic votary of the doctrine of contagion, and ask him whether it be possible, consistent with any known mode of reasoning, for this fever to have been thus suddenly extinguished, if it owed its origin and procreation to a vitiated secretion? Has any alkaline salt ever been known to arrest the morbid action of a contagious fever, even applied directly to the body itself? No pathologist will answer

* Two recruits were admitted in November, in the incipient state of bilious remitting fever. For several days the diseases of both maintained the remittent type, and inflammatory grade, such as marked the disease on the Eastern Shore of the Chesapeake, where they had received the remote cause; but these characters gradually faded away, till they were lost in the more prominent symptoms of genuine typhus.

this question in the affirmative. In the case of the soldiers it was applied only to the bed clothes, and the wearing apparel of the whole corps, and to the walls of their apartments. If the toxicopoetic process was still going on in the vessels, those subsequently admitted should have received the disease, as certainly as those who had previously contracted it from a similar source. The action of the extreme secretory arteries was not cut off, nor even suspended by the act of purification.

It has been intimated by some, of high repute for medical erudition, that perhaps the genuine typhus, such as prevails in Europe, (especially in G. Britain,) is not indigenous to our country. We have yet to learn that the elements are not the same on either side of the Atlantic, and if we respect the opinions of those who have seen and treated the disease in Great Britain and Ireland, as well as in these United States, we may safely affirm, they arise from a like cause, and differ in no essential point. We are not constrained to rely solely on the authority of others to show that the typhus of Europe is not contagious. In November, 1799, the brig Mercury arrived at this port from Cork; her crew was composed of passengers of various descriptions, amounting to forty-four, exclusive of the officers and ordinary seamen. Twenty-three of this number were confined by typhus when the ship arrived, some of whom were indisposed and confined before the brig sailed from Ireland; six had died on the passage, and all who were sick were removed on shore, and were admitted either into boarding houses, or received among their friends in private families. Nine of the twenty-three died on shore, having been nearly exhausted before they arrived. Although every sick man on board was received on shore with that genuine hospitality for which the sons and daughters of Hibernia are so distinguished, not one of them had occasion to lament the consequences of their disinterested and noble philanthropy. If the disease could have been communicated, some one of the hundreds by whom the sick were visited, and more especially those in the families they lodged, must have contracted it. It has been objected to this, and other corresponding facts, that the air of Baltimore was too pure to become the vehicle of contagion; or, in the language of others, to be con-

taminated by the *fomes* proceeding from the bodies of the sick. This word should be banished from the vocabulary of medicine; it is too indefinite to convey any distinct signification; it may be employed with great propriety, to express the force or power of any other agent concerned, directly or indirectly, in the causation of fever. In the sense it is employed by some of the votaries of contagion, it is intended to convey the idea that, in contagious diseases, something emanates from the body which has the capacity of assimilating the atmosphere to its own poisonous nature. This is what has been figuratively styled, by others, "*inoculating the atmosphere*," a misplaced figure, one of the evolutions of a mind undisciplined in the common principles of chymistry. The Supreme Architect has constituted the atmosphere we breathe of different parts, but in a state of chymical combination, difficultly separated by chymical and mechanical means, and never, except in limited portions, altogether unimportant in the general economy of nature. If this *fomes* can assimilate a cubic inch of atmospheric air to its own nature, by the same law it must necessarily multiply *ad infinitum*. Any of the causes of fever may float in the atmosphere in a state of simple mixture with it, but analyze it and it is still the same. Will those who sport such phantoms admit that the atmosphere and contagion are the same, that an animal secretion, inimitable by art, and the effluvium of foul linen, or a marsh, are homogeneous bodies? They are reduced to this dilemma, or their theory is inconsistent. Is there any connexion or similarity of nature between an animal poison and atmospheric air? Can animal matter make such a change in oxygen, azote or carbonic acid, as to assimilate them to its own nature? Atmospheric air acts upon all animal matters, but the power is in the air, not in the animal matter. The animal matter is changed by decomposition, and resolved into its original elements or dissipated. If the atmosphere could thus receive the leaven, and commence a change of its vital properties, it would become irrespirable in the ratio of its progress, till all animal nature would languish and die. The proper sphere of the operation of animal poisons is living animal matter itself; and when it has once been fabricated under the action of a morbid secretion, it reproduces its own specific nature. If it

does not effect this, it does nothing, and therefore is not contagion. We will adduce the authority of the British writers themselves, to prove that typhus has been propagated from sources where there could have been no contagion; and we will select, from a variety of cases, that which has been so frequently and so triumphantly quoted to establish the contagion of typhus. The two very memorable examples of infection at the Black Assizes, at Oxford, in 1577, and the Old Bailey, in 1750, are well adapted to our purpose. As the principle is the same in both, we will prefer the latter, because it is less remote from our own time, and because it is more impartially narrated by a man who was not a physician, and who, for that reason, was partial to no theory.* An abstract of the more prominent parts of the report will suffice. The prisoners, who had been confined in Newgate, were brought into court covered with rags and filth, meagre and squalid in the extreme; *but not sick, for there was no disease in the prison.* During the trial, which lasted two or three days, they stood between the judges and the window, where the current of air sat directly on the bench, as well as the jury, sheriff, and a part of the spectators. The bench consisted of six persons, of whom four died in a few days; sir Samuel Pennant, the lord mayor; sir Thomas Abney and Baron Clark, judges; and sir Daniel Lambert, alderman; three of the counsel; one of the under sheriffs; several of the Middlesex jury, and many of the spectators lost their lives by the effluvium from the prisoners, while the London jury, and all those to the windward of the pestilent effluvium, escaped. May we be permitted to ask in what this pestiferous emanation from bodies not diseased consisted? Not in contagion, the product of diseased vascular action, because it would have been physically impossible for this process to have been effected in animal matter, and no sign of disease to have been discerned nor felt. The very act of secretion in the extreme vessels is itself a fever, and before it can commence, much less be perfected, some of the functions must have been deeply implicated. Can it be possible that a poison so virulent as to smite the judges with death, blast the jury, and

* Foster's Crown Law.—Folio edition, 1762.

poison all those within the sphere of its action, should have been secreted in men, every function of whose bodies was unimpaired and unmolested?

If it be proved that typhus is not contagious, or even rendered questionable, it follows of consequence, that we doubt the capacity of yellow fever to propagate itself. If it is contagious, the secretion by which contagion is generated must reside in some of the secretions. Experiments fairly instituted, and honestly conducted, have invariably eventuated in demonstrating the innoxious quality of all the secretions. The effluvia from the lungs are innocent, because the disease has never been communicated in our hospitals, or other public receptacles of the sick. It is almost nugatory to assert, that no poison can be received into the circulating fluids with impunity; and it only remains for us to show that it cannot be communicated by inoculation, by any of the secretions, that they are inoffensive when brought into contact with the nerves. During the prevalence of the yellow fever in this city, in the year 1797, 1800, and 1802, we proposed and executed several experiments adapted to the question of contagion. On the 20th of September, 1797, having been called upon to visit a man in the act of dying, covered with perspirable matter, I applied a piece of muslin to his body till it become saturated, and in this condition, half an hour after, bound it around my head at bed time. The fetor was nauseous in the extreme, and occasioned a transient sensation of sickness of stomach; but, being languid from excessive exercise during the day, a profound sleep succeeded, from which I did not awake till seven o'clock the next morning. I experienced no further inconvenience.* In 1798, a few sporadic cases occurred in Baltimore. I inoculated myself with the perspirable matter that adhered to the skin of a patient in the last stage of yellow fever, but experienced no other effect than that which followed the punctures made in both arms. As the few cases that occurred this year, assumed an uncommonly malignant

* A similar experiment was made by the late philanthropic but unfortunate Doctor Valli, at the Havanna, in 1816. Although he had breathed the atmosphere of the place in which hundreds had died, his death was ascribed to contagion from the experiment.

aspect, and many of them were accompanied by inguinal buboes, I availed myself of the occasion to inoculate myself with the matter formed by suppuration. On the eleventh of October, I inserted the matter in both arms, taken from the groin of Mr. Rector Lowe. A slight circumscribed redness appeared about twenty-four hours after the operation, and continued nearly stationary for four days. As no irritation proceeded from the punctured parts, and the rubefacience evidently declined on the fifth day, I concluded that the local appearances ought to have been ascribed to the puncture, or the acrimony of the pus.

It has been suggested, that as the stomach is one of the principal organs invaded by inflammation in this disease, and as the black vomit would appear to be a secretion from that organ, that the disease might be reproduced by receiving this secretion into a healthy stomach. The experiments of Dr. Ffirth, have decided that this hypothesis is visionary. He swallowed the black matter in the state it was ejected from the stomach, but with perfect immunity from disease. That this fluid cannot propagate the disease when brought into contact with the nerves of the skin, we have ascertained by reiterated experiments.

It has been argued by some, that inasmuch as the plague has been said to have been communicated by inoculation with the blood, the yellow fever might probably be propagated in the same way. We have no well attested facts to prove that the plague ever has been thus propagated. For reasons already assigned, the opinion is so repugnant to a general law, that we cannot regard it. If the blood in the small-pox cannot communicate that disease by inoculation, no other disease is entitled to the epithet contagious through that medium. The opinion is one of the rotten excrescences of the humoral pathology. We have repeatedly attempted this mode of communication, and have never been disappointed. There is nevertheless a conclusion fairly deducible from the appearances of the blood in this disease, and of those who reside long in the atmosphere where it prevails, that imposes an insuperable objection to the doctrine of contagion. It suggests a palpable distinction between a disease from atmospheric infection, and that which is the offspring of contagion. In September,

1800, a fact was daily obtruded upon the observant practitioner, which, although it had been long familiar to me, had not been duly estimated. It was remarkable, that in all cases in which it was deemed expedient to bleed, the blood wore the same general appearances. After a separation had taken place, the serum assumed a yellow shade, often a deep orange, and a portion of the red globules was invariably precipitated. It occurred to me, that if the remote cause resided in the common atmosphere, the blood of all who had inhaled it a certain time, would exhibit similar phenomena. It accorded with the pathology I had conceived, to conclude that all who lived in an atmosphere so inquinated, were constantly predisposed, and that an additional or exciting cause only would be required to develop the symptoms in form. To ascertain the appearances of the blood in subjects apparently in good health, I drew it from five persons who had lived, during the whole season, in the most infected parts of the city, who were, to every external appearance and inward feeling, in perfect health. The appearances of the blood could not be distinguished from that of those who laboured under the most inveterate grades of the disease. As this experiment might have been considered inconclusive, unless the blood could be compared with that of those who lived in a purer atmosphere, remote from the evolution of miasmata, I selected an equal number of persons who dwelt on the hills in Baltimore county, and drew from each of them ten ounces of blood. The contrast in the appearances was so manifest, that no cause for hesitation remained. There was neither a preternaturally yellow serum, nor a red precipitate; the appearances were such as we find in the blood of healthful subjects. A young gentleman having returned from the western part of Pennsylvania, on the 10th of September, in good health, I drew a few ounces of blood from a vein on that day; it discovered no deviation from that of other healthy persons. He remained in my family till the 26th of the month; and, on that day, I repeated the blood-letting. The serum had assumed a deep yellow hue, and a copious precipitate of red globules had fallen to the bottom of the receiving vessel. Of the six persons whose blood assumed those indications of the remote cause, four were seized with yellow fever during

the epidemic; the other two escaped any formal attack, but complained occasionally of head ach, nausea, and other nascent indications of disease. Hundreds who were not confined, and who never took medicine, experienced the effects of the general cause, under a variety of forms, such as nausea, giddiness, head-ach, constipation, a pale or yellow face, tinnitus aurium, pains in the extremities, and some other light shades of incipient indisposition. In some these were premonitory of a formal attack, in others they vanished, leaving the subject in his usual health. Many were listless, complaining of universal languor, indisposed to muscular exertion, yet did not surrender to the disease. They were neither sufficiently indisposed to be placed on the sick list, nor well enough to pursue their ordinary occupations. May we be permitted to ask of our antagonists, a rationale of those phenomena consistent with the modus operandi of contagion? If the cause can pass out of the body without producing the disease, the fact at once draws a line of distinction between infection and contagion, that the unerring laws of nature will not permit to be violated. If the matter of contagion once impinges on the nerves, disease, in all its parts, awaits the subject on which it acts, and his destiny is fixed. In the operation of contagion there is no medium, semina sown in embryo multiply in incalculable progression, till their influence is arrested by art, or death; unless in the few instances which in the energies of the system resist the morbid force, and permit the vital functions to be restored. This position is true of all contagious fevers, and will avail us in accounting for the otherwise inexplicable fact, that no contagious fever can be effectually impressed a second time upon the same body. The first assault of every such disease, by an universal law, (through the goodness of the Almighty,) unfits the subject for a repetition of the same mode of action, and is a guarantee for life. Can this notorious fact be predicated of the yellow fever? If the visionary hypothesis, that this disease could be impressed upon the same system but once, were founded in truth, I might not have been doomed to have lamented a shattered constitution before the meridian of life. In August, 1795, my own was one of the few sporadic cases that occurred in Philadelphia, and was treated by my illustrious pre-

ceptor Dr. Rush. In July, 1796, I suffered a more formidable assault, and was again restored by the same. In September, 1800, I experienced a still more inveterate grade of the disease, then epidemic in this city, and was safely conducted to health by the skill and unremitted attention of Dr. Alexander. There are even some instances of two attacks of yellow fever in the same person, during the same summer and autumn, some of which I have treated. It would be superfluous to pursue an hypothesis, long refuted by the experience of my countrymen. The best commentary, on this sophism, is written with the tears of the widow and the orphan, of every sea port in the Union north of Charleston, South Carolina.

FOREIGN PAPERS, &c.

On the Cure of Whitlow by Compression. By William Balfour, M. D. of Edinburgh.

[From the London Medical and Physical Journal.]

Two modes of curing whitlow have prevailed, the very opposite of each other: the one tends to promote, the other to prevent, the formation of matter. Such diversity of practice must have originated, one would think, in discrepancy of opinion, with regard to the disease being disposed to run a certain course. No man, surely, who thinks that the formation of matter can, with propriety, be prevented, would advise emollients; none, on the other hand, would prescribe astringents, who believed the formation of matter necessary to the cure of the disease.

The emollient plan of treating whitlow is now, I believe, generally laid aside; and, though the practice of applying astringents rests on sound principles, it is not efficient. It is but now and then, and in slight cases, that astringents will arrest the progress of the disease. If distention was confined to superficial vessels, as in slight cases of sore throat, astringents would quickly prevent the formation of matter in most instances. But, as the complaint is often deep-seated, it is impossible that the most powerful astringents we possess can have much effect in diminishing the calibre, or in strengthening the tone of the distended vessels.

But it is not very evident, that the principle on which astringents produce their beneficial effects in whitlow has had its due consideration. If it had, we surely should not hear of some, whom we are bound to consider among the best informed of the profession, applying them warm. Heat must increase the disten-

tion of vessels already over distended, and produce that effusion which it is the object of the practitioner to prevent.

My success in curing and relieving rheumatic complaints, by merely unloading and supporting the vessels of the parts affected, as detailed in my treatise on that subject, suggested to me the practicability of obviating and removing other inflammatory affections by the same simple means. This practice, therefore, I have adopted in whitlow, and with all the success that could be desired. A careful observation, indeed, of the phenomena exhibited in the first stage of whitlow, (and to this stage alone are my observations confined,) naturally leads to compression as the most likely means of arresting the disease. When the pain of incipient whitlow first excites a patient's attention, he perceives, in the part affected, a degree of tension which he did not observe before. An accurate observer must, therefore, connect in his mind, this tension and pain, as cause and effect. In this he must be confirmed, on observing, that, with tension, pain also increases. We know, indeed, from experience, that tension is the cause of pain: for, no sooner are the vessels of the affected part unloaded, by leeches or the scalpel, than pain abates in proportion. It is evident, therefore, that whatever is capable of restoring the balance between the arteries and veins, must arrest the progress of the disease. This compression can do, as illustrated in the following cases.

Case 1.—About two years ago, Janet Bain, aged thirty-five, a servant, was seized with whitlow in one of her thumbs, after having been employed a whole day in the bleaching-green during very cold weather. When she applied to me, the whole thumb was swelled, painful, and of a livid colour; she knew no cause for her complaint. I never saw any case of whitlow of so deep a colour, and apprehended the very worst issue. I applied compression with the hand for a considerable time, and then with a bandage. I had little or no hope, however, of these means proving of much advantage. From the pain and appearance of the thumb, I was afraid that mortification would quickly supervene. The patient did not call upon me again for some days, when I found her thumb perfectly well. The family surgeon had called soon after she left me, and she was ordered to put herself under his care. The

bandage I applied being still about the thumb, she satisfied herself with a verbal account of the pain and appearance, without informing him she had consulted any one. The surgeon advised a warm vinegar poultice. With this, the patient would not comply; but agreed to adopt so much of his advice only, as she imagined was not inconsistent with my directions. She, therefore, dipped her thumb in cold vinegar, without undoing the bandage. This had all the effect of tightening the bandage, without undoing it; and the cure was thus speedily completed.

Case 2.—Towards the latter end of 1816, a lady received a severe blow on the middle joint of her middle finger, right hand, from the falling of a window upon it, the pulley of which was broken. The pain, which was extreme, soon abated, but was followed by swelling of the whole finger, extending over a considerable portion of the back of the hand, especially in the course of the extensor tendon of the finger. A fortnight elapsed, from the evening of the accident, before I was consulted. During this period, the tension and pain had gradually increased, and were now extreme. The whole of the finger and back of the hand were pained, but chiefly the middle joint and first phalanx; and some idea may be formed of the distress of the patient, from the circumstance of my being called to her, for the first time, at eleven o'clock at night. The tension, throbbing, and pain, were insufferable, and must soon have terminated in suppuration. I applied compression over all the parts affected, with my fingers and thumb; at first in a very gentle manner, and gradually with more freedom, as the feelings of the patient would permit. By these means, I reduced the swelling perceptibly in a short time, and then applied a narrow fillet circularly the whole length of the finger, and with a degree of tightness which could not have been suffered without the previous handling, and which was still regulated by the patient's feelings. I directed her to keep the joints of the fingers a little elevated during the night, and to slacken the bandage if necessary. Next day, I found she passed the night comfortably, without having occasion to undo the bandage; and that both swelling and pain had greatly abated. All the symptoms, indeed, were so much ameliorated, that the patient insisted on omitting

the bandage. On the following day, the second from the time I was called, she was menaced with a return of pain. I was again sent for, and renewed my operations. In a day or two the pain ceased entirely and permanently.

Case 3.—About twelve months ago, a young lady was seized with whitlow in one of her thumbs, without any assignable cause. After suffering a gradual increase of pain for ten or twelve days, she became, at last, quite impatient, and consulted me. I found the whole of the last phalanx tense, red, shining, and excruciatingly painful, especially about the root of the nail. The whole hand was in some degree affected; and the pain shot as high as the elbow-joint. On examination, I told her, suppuration had either taken place, or was on the point of it; so that she was rather late in applying, for my mode of treatment to take effect. I applied compression, however, with the hand, in a degree she could easily bear, with the view of preventing extensive suppuration, and then a narrow fillet. This operation was repeated three or four times in the course of two days, when pain and swelling disappeared, leaving a single speck of pus at the point of the thumb, immediately under the skin;—to this, I gave vent by the slightest touch of the lancet, and the wound healed up immediately. That the formation of pus was checked, by these operations, after it was begun, there cannot, I think, be a doubt.

Case 4.—Colin Mackenzie, aged forty-six, applied to me for advice, on the 6th of August, 1817, on account of the little finger of his right hand, which was swelled and excruciatingly painful, from the middle joint to the point inclusive. He knew no cause for the complaint, which had been gradually increasing for eight days, and now prevented him entirely from following his occupation. These symptoms, and the glossy appearance of the finger, left no room to doubt that this was a case of severe whitlow. I applied compression over all the parts affected, with my finger and thumb, moved from place to place in succession. The joints, indeed, were so very painful, that the patient could scarcely suffer them to be touched. But, proceeding leisurely and with caution, I overcame this tenderness in about fifteen minutes. I then bound up the finger its whole length, with a piece of narrow tape,—re-

gulating the degree of compression by the patient's feelings. This took place at eight o'clock in the evening, and I directed him to slacken the bandage before going to bed, if he found it too tight. This he did, and also next day, while at work. He called on me again at eight o'clock in the evening of the 7th, twenty-four hours after the first operation, when I found the swelling generally gone, and the pain greatly abated, except at the middle joint, which was originally most painful, and to which the patient did not re-apply the bandage sufficiently tight when he had occasion to undo it. This joint was, therefore, more swelled and pained than ever. I applied compression to it as before, till the patient admitted that both tension and pain were relieved; and then the bandage. Next morning, at ten o'clock, the tension and pain were so nearly removed, that almost complete flexion of the finger was restored. Thus, in the short space of thirty-eight hours, was this man relieved from a complaint, which, under the usual treatment, must infallibly have ended in suppuration and protracted suffering.

Case 5.—James Briddet, a tanner, aged twenty-five, applied to me on the 25th of August, with whitlow in one of his thumbs. He knew no cause for the complaint, which had existed a week, and was now likely to prevent him following his occupation. The whole thumb was affected, but particularly the two joints; and the inflammation about the first joint and ball was such as to make me observe to the patient, that certainly the parts were coloured by the substances he had occasion to handle when at work. He assured me the appearance of the parts was entirely the effect of disease. I found this fellow quite sceptical with regard to my mode of cure. When I had handled the parts and applied a bandage, I desired him to call next day. He looked at me, as if he would have said, "Is this all you are to do for me?" He called next morning, however, when inflammation and swelling considerably abated, especially about the first joint. The last joint was still painful. In the evening of the same day, this joint was also much better. Next day the third inclusive swelling and pain were almost entirely gone; and the patient had the free use of his thumb. I now asked him, if he was not, at first, quite distrustful in the mode of cure I adopted? He admitted he was, expressed

his surprise at the result, made his acknowledgments, and went about his business.

Case 6.—Peter Fraser received an injury on the 26th of December last, by having his left thumb bent forcibly backwards, when assisting at lifting a heavy stone. When he applied to me on the 29th, he complained of having passed three days in great agony, and three sleepless nights. The pain was confined to the first joint, but the swelling extended a considerable way upwards. I never handled a more excruciatingly painful case, and I believed it must soon have terminated in suppuration. Such was also the opinion of Dr. Anderson, of New York, who happened to be with me when the patient presented himself. I told that gentleman, that, exquisitely painful as was the complaint, I had no doubt of curing it in a week, without any other application than my own fingers and a piece of narrow tape. The cure was completed in six days, inclusive of that on which the patient applied to me.

I shall make some observations on these cases hereafter; in the mean time, I would say of the mode of cure, "*Si quid novisti rectius istis, candidus imperti; si non, his utere mecum.*"

January 8, 1818.

Case of Recovery after the Separation and Discharge by Stool of a Portion of the Ileum. By Alexander Renton, Surgeon, Penicuik.

[From the Edinburgh Medical Journal]

I have been induced, from the singular nature of the following case, to request its insertion in your Journal. I am not aware that the annals of Surgery contain the like history of a similar affection. Cases indeed are recorded, wherein portions of the intestinal canal have spontaneously sphacelated, and been discharged by stool, and the process of digestion has still continued to go on; yet, so far as I know, the salutary efforts of nature in such

cases have not been found to be permanent and complete; the patients sooner or later have sunk; and their existence seemed rather to prove, that it was possible that nature might survive for a time, than that she could eventually recover from, the partial destruction of so important a viscus. From a knowledge, therefore, of the unfortunate termination of these affections, I have been prevented communicating to you the history of this one, until I saw its ultimate result, by which I might be enabled to speak with more certainty of all the circumstances connected with it.

G. B. who is a mason by trade, went to his work at six o'clock on Saturday morning, July 6th, 1816, in good health, and was suddenly seized in about an hour and a half afterwards with pyrosis, and pain in the left hypochrondrium, with a violent drawing in of his bowels, followed by a sensation as if some portion of them was squeezed and constricted. Thinking to alleviate the severity of the pain, he ran immediately to stool, and procured a motion, without the relief he anticipated from it. He was carried to a neighbouring village, and, on his swallowing a glassful of spirits, vomiting, for the first time, commenced. There he was bathed in warm water, the pain continuing unabated. Through the course of the day, castor oil, at different times, was given him, which his stomach uniformly rejected. By his own desire he was taken home in a cart, a distance of nearly four miles. I saw him about ten o'clock in the evening. He was apparently in great distress; his body was bent upon his thighs; there was tension, increased heat, and pain of the abdomen, especially below the umbilicus, aggravated by pressure, and constant, though not always equally violent. He was restless in every posture; a profuse perspiration covered the surface of his body; his pulse was quick and strong, and he was frequently straining for a motion, but without effect.

I took twenty-four ounces of blood from his arm, and directed that he should be put immediately into a warm bath, and that stimulating enemas be repeated every hour, with an ounce of castor oil, until some effect from his bowels be procured. I saw him next morning by eight o'clock, and found him much in the same state; his belly on the whole being more generally tense, though the pain

occupied a particular part of it, to which a bladder of hot water had constantly been kept applied. I bled him again to the same extent, and learnt that the castor oil he took through the night had been rejected, and that the injections had come away without effect. He was again put into the warm bath, and experienced in it the same temporary relief he received before. His stomach becoming more irritable, I ordered, in place of the castor oil, small doses of scammony, alternated with aloes and the injections, every two hours. At bed time the warm bath was repeated, which he urgently cried for, and a large blister was applied to the abdomen. The medicines he took remained for a considerable time upon the stomach, probably by the stomach and duodenum being emptied of their contents by vomiting, and the feculent discharge not having yet commenced. On Monday morning he was again put into the warm bath, out of which he was taken in about ten minutes, in a state of syncope. During the latter part of the night, he began to reject large quantities of crude and indigested matter, which appeared from its smell and consistence to be the contents of the small intestines. When I asked him about the affected part of the belly, he laid his hand upon a spot from which he said all his distress proceeded. He has taken eighteen grains of scammony, and twelve common aloetic pills, some of which were found entire in what he had vomited. Instead of these purgatives, I recommended him to try the effects of Epsom salts, with the compound powder of jalap, in separate doses, and to assist their operation by a stimulating clyster in the evening. I saw him again at four in the afternoon, and was informed that all the medicines were vomited, and that the pain had not intermitted in violence. His strength was now much exhausted, and a cold clammy sweat pervaded his body. With much difficulty he could turn himself in bed. He had frequent singultus; and when he was attacked with a violent spasm, his voice seemed to forsake him. In this very hopeless condition of our patient, I recommended to him a more powerful cathartic, and he was very willing to grasp at any proposal that afforded the slightest promise of relief. Accordingly, six grains of the mild muriate of mercury, with an ounce of the ol. ricini, were ordered to be taken every two hours,

until I heard of their effects. By seven, on Tuesday morning, he had taken seven doses of each of the medicines, and none of them had been rejected, when nearly eight pints of dark coloured bloody excrementitious matter were discharged by stool. He now felt much relieved, the ease that followed the evacuation being almost as sudden as the pain in the commencement of the attack had been. The tension and pain of the abdomen subsided, and through the course of the day he had several copious stools, which were very fetid, and but slightly stained with blood. During the thirteen subsequent days, he continued free from any urgent symptom, excepting the occasional occurrence of spasms in the part originally affected, which seemed to proceed from costiveness, as they were alleviated by the exhibition of a glyster and castor oil. He had occasion to have recourse to the warm bath twice only in that period, in order to mitigate a sudden attack of pain in the abdomen.

On the morning of the fourteenth day after the discharge, he became restless, and all the former symptoms relapsed in their most aggravated form. In this state he continued for three hours, racked with violent bearing down pain, by endeavouring, in straining, to expel something from his bowels, that required all his efforts.

From this distressed state he was as speedily freed by the discharge of a portion of the ileum nearly eighteen inches in length. Worn out with pain, he fell immediately into a profound sleep for three hours. Since that time he has recovered gradually from the effects of his complaints, and has been for several months past at his usual employment. He is about forty-five years old, of a lank and relaxed habit, and strongly marked with all the symptoms of the leucophlegmatic temperament. Upon inquiring into some of the circumstances of his complaint, I was told by him, that he had been raising, on the morning of the attack, several heavy weights, and that he attributed the cause of it to the exertion he used.

The nature of the case, from the preceding detail, appears to be sufficiently obvious, and can in no other way be accounted for, than by supposing, that one portion of intestine had descended within another, had there become strangulated, and that the contiguous

sides of the intestine, in the upper part, where the stricture existed, had united by the adhesive inflammation, while the included portion of it had been separated by the process, by which dead parts are removed from living surfaces.

A considerable part of mesentery was attached to the portion of ileum which was expelled; the division of the vessels of which may account for the flow of blood, that appeared only to an alarming extent in the first evacuation, and which seemed indeed to consist principally of that fluid.

It is likewise evident, that the introsuscepted intestine must have been, at least, partially separated thirteen days before it was finally expelled, as he had, during that interval, a free discharge of excrement, without much difficulty or pain. Whether or not it may have remained in the large intestine, for any length of time after it was separated, becomes a question that does not seem to admit of ready explanation.

Among many symptoms that more particularly harassed him after he was free from the immediate danger of the attack, was a frequent craving or hunger, which was not gratified wholly by the taking of food. He was nourished for a long time upon the simplest and mildest diet; and it required no inconsiderable share of attention on his own part, to select those articles of food which he found by experience to agree best with him.

Upon taking animal food, or any other form of diet which disagreed with him, he could tell with a good deal of precision, the time when he should be attacked with pain in consequence of it. This generally happened in about three hours afterwards.

Costiveness was likewise an effect of the complaint, which, unless obviated by the almost daily use of laxatives, never failed to reproduce an attack of pain, during the progress of his tedious recovery, in the primary seat of the disease.

The only other fact worthy of record is, that, during his convalescence, his bowels remained for a long time in a very irritable state, so that wine and other cordials, recommended for his weakness, were obliged to be relinquished on account of their stimulating effects upon the intestines, and that, even yet, malt and diluted spirituous liquors are attended with similar consequences.

I have only to mention further, in concluding the history of this case, that the introsuscepted portion of ileum was given to Dr. Thomson, professor of surgery in Edinburgh, and, I believe, is now to be seen in the Museum of the Royal College of Surgeons of that place.

Penicuik, August 6th, 1817.

On the Use of the Prussic (Hydrocyanic) Acid in the Treatment of certain Diseases of the Chest, and particularly in Phthisis Pulmonalis. By Dr. Magendie. (Read at the Royal Academy of Sciences of Paris the 17th November, 1817, and communicated exclusively to the Editor of the Journal of the Royal Institution.)

[From the Journal of Science and the Arts, for 1818. No. VIII.]

My Dear Sir,

The very distinguished author of the subjoined memoir, well known for his zeal in promoting physiological science, has transmitted the original to me, immediately after its having been read before the Royal Academy of Sciences at Paris, with a request that I would translate it, and publish it exclusively in the Journal of the Royal Institution. The well merited reputation which this Journal has gained among the learned of the continental nations, for its candour, impartiality, and the originality of some of its valuable papers, will explain this preference in my friend Dr. Magendie; and I feel happy in the opportunity of saying, that it is his intention, and that of many other eminent characters in Paris and in Italy, with whom I have conversed on the subject, occasionally to favour the Journal with their original communications.

On the subject of the following pages I might descant a little, were it not superfluous to repeat what the author himself has so clearly stated. Its importance is too evident to need any recommendation. But you will perhaps allow me to say, that having been present at some of the experiments; conscious as I am, from

actual examination during my residence in Paris, of the truth of many of the facts brought forward; and enjoying the pleasure of intimately knowing the author, to whose sagacity and talents I shall be happy on all occasions to do justice, I cannot hesitate in recommending Dr. Magendie's paper to the earnest attention of all my medical brethren.

Yours truly,

A. B. GRANVILLE.

8, Saville Row, December, 1817.

PHYSIOLOGICAL experiments are of the utmost importance in the practice of medicine. It is by means of them, that substances used as medicines on mere hypothetical principles are justly rejected, and the really active remedies better known, or more beneficially employed, by varying their proportions and mode of administration. Another great advantage resulting from physiological experiments, is the discovery of new remedies, which the physician is enabled to make by their assistance, and by directing his attention to substances already known, but either neglected or seldom used, as well as to those preparations, which modern chemistry is daily bringing before us, and which, in the hand of an able and experienced practitioner, may become particularly useful to mankind.

It was on account of this firm persuasion, that I have ventured, at different periods, to call the attention of the Royal Academy of Sciences to the poisonous and medicinal qualities of the *Upas tieuté*, the *nux vomica*, the *supertartrate of antimony and potash*, and the *ipecacuanha*. The favourable reception which my researches have met with, have encouraged me to proceed farther, and, I shall have the honour this day of laying before the Academy, the result of my inquiries respecting the action of the prussic or hydrocyanic acid on the animal system, and the good effects which may be derived from it in the treatment of several diseases.

The prussic acid, discovered in 1780 by Scheele, was soon afterwards reckoned amongst the poisonous substances; an opinion

which was confirmed by the experiments made, both in Germany and France, by Messrs. Coulon, Emmert, Robert, Orfila, &c.— From their experiments, and from some which are peculiarly my own, it results; 1st, that the prussic acid, whether liquid or in the state of vapour, is injurious to the life of all animals, and in many cases, to that of vegetables; 2d, that death produced by this acid is much more instantaneous, from the circulation becoming more rapid, and the lungs more distended; 3d, that it acts on warm-blooded animals by destroying their sensibility and the contractility of the voluntary muscles; and would act in a similar manner on man, where the dose given is greater than what will be hereafter mentioned.

It cannot be denied, therefore, that the prussic acid is a very active poison; and yet all the experiments I have just now alluded to, have been made with the prussic acid of Scheele, which contained a very considerable quantity of water, and was consequently very weak. From this circumstance it will be easy to conceive, that the effects of the *pure* acid, prepared according to Gay Lussac's directions, must need be much more energetic. Its activity, indeed, is really frightful, even to those who are accustomed to witness the effects of poison. Of this the Academy shall judge by the following facts.

Experiment 1. The extremity of a glass tube which had been previously dipped into a phial containing some pure prussic acid, was immediately plunged into the throat of a strong dog. The tube had scarcely come in contact with the tongue, than the animal made two or three long and rapid inspirations, and fell dead. No method we could devise enabled us, afterwards, to trace the smallest sign of sensibility in the muscular organs of this animal after death.

Experiment 2. An *atom* of the acid was applied to the eye of another strong dog; the effects were as sudden and as fatal as in the preceding experiment.

Experiment 3. A drop of the acid diluted with four drops of alcohol, were injected into the jugular vein of a third dog. The animal fell dead, that instnnt, as if struck by *a cannon shot or by lightning*.

In short, the pure prussic acid, prepared according to M. Gay Lussac, is without doubt, of all the known poisons, the most active and the most promptly mortal. Its deleterious and powerful influence permits us to believe, what the historians have related, of the criminal talent of Laucustus; and render the accounts of those extraordinary and sudden cases of poisoning, so frequent in the annals of Italy, less marvellous and incredible.

I must not omit to say, likewise, for the sake of those who should feel disposed to make experiments with this substance, that the utmost caution is necessary; as it is impossible to breathe its vapour without feeling the most dangerous effects. Owing to some neglect on our part, in this respect, we have at one time experienced the most excruciating pain in the chest, accompanied by a feeling of insupportable oppression, which lasted some hours.

It might appear from all this that the pure prussic acid in the hands of one intent on murder, would become the instrument of crime, without fear of detection; but the public may make themselves easy on that score. More nicety of manipulation, and dexterity in the operations of chemistry is required to procure this acid, than falls to the lot of common people; and even when properly prepared, it is almost impossible to preserve it in its state of purity, as I have ascertained by direct experiment. It is spontaneously decomposed at the ordinary temperature of the atmosphere, and then loses, in a short time, all its obnoxious qualities. Besides, though the prussic acid produces death, without any visible alteration of the animal organs, it is easy to detect a case of poisoning by it: for the body will exhale, during several days, a strong smell of bitter almonds.

Poisonous as it is, there is no doubt but that the prussic acid may, when properly diluted with water, be used as a medicine with safety. We know from the experiments which Mons. Coulon made on himself, that it may be given to the dose of sixty drops without producing any very serious inconvenience. Besides, the pretty frequent use made in medicine, of the laurel water, in which the prussic acid enters, as a component principle, proves that it may be introduced into the stomach when properly diluted. Nothing, therefore, shews any impropriety in its use as a remedy;

a circumstance which has already induced some French and Italian physicians to give it in various disorders. If their success has not been equal to their expectations, it is because they did not seem sufficiently aware of its mode of action on the animal economy; and without this knowledge it is impossible to make a right use of any new remedy whatever.

In studying the phenomena of poisoning by prussic acid, I have often observed, that animals, in which no trace of sensibility, or muscular contractility could be found, would often continue to breathe for several hours, freely, while their circulation, though much accelerated, remained apparently unaltered. These animals indeed might have been said to be dead with regard to their external functions, yet still enjoying life, through their nutritive faculties.

This property of extinguishing the general sensibility without any ostensible injury to the respiration and circulation, the two principal functions of life, induced me first to believe that the prussic acid might be advantageously used in cases where the disease seemed to owe its origin to a vicious augmentation of sensibility. From that moment I determined to use it whenever any such case should offer itself to my attention.

About three years ago I was consulted in behalf of a young lady aged 27, who for the space of eighteen months, had been distressed by a dry short cough, which became stronger in the evening and in the morning. Alarmed at these symptoms, which seemed to indicate an affection of the lungs, her friends took the advice of several of the most distinguished medical practitioners of the Capital, who all prescribed the usual remedies in such cases, but without success. I ordered her six drops of Scheele's prussic acid, prepared by Pelletier, diluted with three ounces of a vegetable infusion; to be taken by spoonfuls every two hours. The following day the cough had considerably diminished, and it disappeared entirely on the fourth.

The cough however having made once more its appearance six months afterwards, I repeated the same remedy with an equal success.

Since then I have had repeated opportunities, but chiefly with

young ladies, to employ the prussic acid in cases of nervous and chronic coughs; and have always obtained the greatest success, without having observed any inconvenience from it. In no case have I gone beyond the dose of *ten* drops taken at intervals during twenty-four hours, and diluting it with several ounces of some fluid vehicle.

Very lately I have succeeded in calming by this same means a convulsive cough, with which an elderly lady of a nervous temperament had been greatly affected, and which for six days previous to my seeing her, had come on by alarming fits, depriving her of all rest. I was so much the more willing to adopt in this case the use of prussic acid, as the patient could take neither opium nor any preparation of poppies without being grievously incommoded.

After thus having ascertained the efficacy of the prussic acid in the treatment of dry convulsive cough, I thought it was indispensable for me to inquire whether the same means might not be employed with success to combat the cough and other symptoms which overpower the unhappy consumptive—and whether it would not influence, or even suspend the progress of pulmonary consumption.

The result of my trials has been favourable with regard to the first of these conjectures; and on fifteen persons, affected with phthisis, who had been placed under my care for the last three years, I have constantly found that the use of the prussic acid, given in small but repeated doses, diminished the frequency of the cough, moderated, and rendered more easy the expectoration, and lastly, procured the patients some sleep at night without any colliquative sweats. Those who are accustomed to follow the march and progress of phthisis, and witness the sufferings without number, by which individuals attacked by this terrible malady are overpowered, will easily appreciate the real benefit of this success.

Since the beginning of the month of August last to this day, (November) I have had many opportunities of studying the effects of prussic acid on a great number of phthisical patients at the hospital of *La Charité*. Mons. Lerminier, physician to that hospi-

tal, in which such diseases are very frequent, has, at my request, agreed to administer the prussic acid in about twenty cases, at the dose of four drops properly diluted with water.

The greater number have shewn evident signs of amelioration, and some seem much better at this moment. The cough is considerably diminished. The expectoration has become easy, and sleep came to shorten their sufferings. These improvements became more evident, where the disease was in an incipient state; a circumstance which is not difficult to explain, when it is considered, that the lungs are in a state of disorganization, in the second, and above all, in the third stage of consumption.

Yet as I wish to state merely, in this place, the *exact* effects of the prussic acid, I must avow that amongst the patients of *La Charité*, who have used it, some, whose disease was near its end, did not derive any very sensible benefit from it; and that in two instances, in which the patients had taken the acid at too short intervals, experienced some headach, and a kind of vertigo which lasted some seconds. In a third case it was feared that the acid had proved injurious.

A young man aged 29 was admitted into the hospital towards the end of September last, with all the symptoms of a confirmed phthisis.* The acid was given in the dose of six drops only. The cough diminished the second day; but the oppression increased; the third day it became suffocating, though the acid had been discontinued the preceding day; and the patient fell into a sort of insensibility, which terminated in a few hours in death. This could not have failed to raise some suspicions against the remedy, if the examination *post mortem* had not clearly shewn, that the fatal termination of the malady was owing to an immense quantity of serous fluid found in the left side of the thorax; the heart being thrown to the right, and touching the ribs of that side. Neither the stomach nor the body exhaled any prussic smell. The disease in this case was in its second stage; but it was evidently not the principal dis-

* Contrary to the established practice of Mons. Lerminier, the chest was not explored by means of percussion, as generally done with all the patients who enter at *La Charité*. This prevented the discovery of an old inflammation of the pleura which affected the left side of the chest.

ease by which the patient was affected, a circumstance which will account for the absence of all sort of beneficial results from the prussic acid.

In my private practice I never observed any bad effect to result from the action of the prussic acid given as I directed, which may be accounted for by considering the great care and attention which a patient in the bosom of his family receives at every moment; the case being necessarily different in regard to patients in even the best regulated hospitals.

From all that precedes I think I am warranted in concluding, that the prussic acid, given in small doses, mixed with a certain quantity of water, may be advantageously employed as a palliative treatment of consumption, with a view of calming the cough, facilitating the expectoration, and procuring sleep; and that as such it must be considered as the first among the substances usually employed for similar purposes; as it does not seem to excite, like the opiates, any colliquative sweat.

It still remains to inquire, whether by the assistance of the prussic acid and of its marvellous activity, we might not hope to render the march of phthisis more slow, and even to cure it. But these questions, in themselves so important on account of the too fatal prevalence of the malady, cannot be decided by a small number of facts and experiments. They ought, on the contrary, to be multiplied as much as possible, taking at the same time into consideration all the circumstances which might influence the results; and divesting ourselves of all sort of prejudice.

I am continuing my experiments on this subject, conjointly with M. Lerminier, at the hospital of *La Charité*, where from twenty to thirty consumptive patients are habitually received; and I am in hopes to lay before the Academy, in the course of the next year, some facts worthy of their attention.

Some may consider it as a mark of temerity in me to suppose that phthisis may be cured, when so many very eminent authors look upon it as absolutely mortal. But admitting that phthisis be incurable, with reference to all the substances hitherto employed, and the experiments hitherto made for that purpose in the true spirit of truth and investigation, the same mode of reasoning can-

not hold good with regard to new substances, remarkable for the energy of their action on the animal economy: besides, ought not the physician to direct his attention to the cure of such diseases as *phthisis*, *cancer*, &c. in preference to merely varying the treatment of such diseases as, from peculiar circumstances and their nature, terminate always happily, whatever be the remedy suggested by fashion or caprice?

I shall now relate two cases in which there is reason to presume that phthisis was checked in its progress by the use of prussic acid.

Case 1.—A lady from Lyons, now residing in Paris, of a constitution eminently bilious, after having experienced several misfortunes, was, in 1814, attacked by all the symptoms which characterise phthisis in its first stage. Circumstances not allowing her to attend to her health, she neglected it, until within the first months of 1815, when the disease having made great progress, she consulted me: I found her labouring under all the symptoms of the second stage of tuberculous consumption, with a cough returning incessantly, and a slow continued fever preying upon her and undermining her existence. The prussic acid was recommended, and taken at the dose of from six to ten drops in twenty-four hours, diluted. This acid had been prepared by Mons. Planche, by a new process which I shall hereafter describe. The remedy was continued for about two months. From the first day the cough diminished, the patient slept, and without pushing the dose higher than ten drops in the twenty-four hours, all the symptoms of the disease disappeared, the breathing became natural, the cough, expectoration, and sweats ceased. In short, the lady was perfectly cured, and has never since experienced any symptoms, which indicate the least disposition to a relapse. Her lungs, only, have become very sensible to the influence of atmospherical variations.

Must we conclude from this fact, that a consumption in its second stage has been cured by the prussic acid? I am far from thinking so; for I know with how much reserve we ought to draw any general and positive conclusion. Yet, such as it is, and with

all the importance attached to it, I submit the case to the practitioners, who take an interest in the progress of science.

Case 2.—An English lady, aged twenty-eight, tall, and of a weak constitution, with a chest transversely ample, but narrow, was frequently subject to colds from her infancy. Last year, while crossing over from France to England, she was attacked by an inflammation of the lungs, with acute pain of the left side of the thorax, and spitting of blood. Bleeding, blisters, and all the means usually adopted in such cases, were had recourse to, and she got well; but she continued to suffer from a short dry cough, of no great intensity during the day, but which became greatly exasperated in the night. Several means were employed in England to check it, with no success. Believing that the climate of France might prove more beneficial to her health, she returned to Paris about four months ago. In spite of the fineness of the season, and residence in the country, the cough made considerable progress; she became uneasy and alarmed; and I was sent for about the middle of September last; when, after a mature examination of the preceding circumstances, and her actual state, I did not hesitate in considering her as labouring under the first stage of pulmonary consumption. I prescribed the prussic acid, prepared by M. Planche, in the dose of eight drops in three ounces of water in twenty-four hours. She has continued it ever since, and is at this day taking it. Her cough has nearly vanished altogether; she has gained considerable *embonpoint;* and she considers herself at this moment as quite recovered.

Far be it from me to suppose that this is another instance of pulmonary consumption cured by the prussic acid. Yet it must be confessed that if examples like the two preceding ones were to become numerous, nothing could then prevent us from hoping, that we may at last have found a substance capable of arresting the progress of one of the most desolating maladies by which humanity is afflicted.

I shall conclude with a few words on the mode of preparing the prussic acid. If it be prepared in the way directed by Scheele, we run the chance of obtaining it at various degrees of concentration—a circumstance which ought by all means to be avoided.

M. Planche, one of the most distinguished *pharmaciens* of Paris, follows a method which renders its preparation less uncertain. Instead of drawing one-fourth of the produce by distillation, as the illustrious Swedish chemist first suggested, he stops the operation, as soon as one-sixth of the produce has passed into the receiver. He next rectifies the liquid thus obtained by means of a gentle fire, over $\frac{1}{200}$ of carbonate of lime, and draws off three-fourths only of the whole by distillation.

Thus prepared the prussic acid is certainly far from possessing the activity of that prepared by Gay Lussac; but it has more energy than that of Scheele, and above all, it has an *invariable* energy; since, according to M. Planche, it is always at the same degree of concentration. It also affords one other advantage, that of being susceptible of preservation, if kept in a cool place, equally removed from the influence of air and light.

I have also employed in many cases the acid prepared according to Scheele's method, and have not been struck with any difference in its action, (such as the theory of chemical doctrine seems to suggest,) whenever I have taken care to get it at the houses of our best chemists.

The observations in this paper are,

1st. That the pure prussic acid is a substance eminently deleterious and altogether unfit to be used as a medicine.

2dly. That the prussic acid diluted with water is beneficial in cases of chronic and nervous cough.

3dly. That this same acid may be useful in the palliative treatment of phthisis, by diminishing the intensity and frequency of the cough, and in procuring sleep, &c.

4thly. That there is reason to hope that this same substance may become advantageous, as a curative treatment of pulmonary phthisis, particularly when in an incipient state.

Singular Treatment of Retroverted Uterus.

[From the Continental Medical Repertory.]

Retroversio uteri gravidi cum prolapsu vaginae. A woman, forty years of age, half gone in her pregnancy, had fallen down stairs a month ago, upon the os coccygis, the vagina had prolapsed to the left side, the uterus was retroversed backwards, the urine only went off guttatim when the prolapsed parts were removed, and the urinary bladder was much distended. At the same time she suffered violent pains in the abdomen, with want of appetite, febrile pulse, anxiety, and low spirits. The reposition did not succeed in the usual way, but the author found out an instrument for that purpose, which is represented in the plate. It consists of a steel staff bent to the axis of the pelvis, terminating at its lower end in a wooden handle, and at its upper end in a blunt cone of cork, covered with soft leather, and which is concave at its upper surface. Having made the woman support herself upon her knees and elbows, he introduced the instrument, directing it with the first finger of the left hand, towards the posterior part of the vagina, so as to have the convexity of the staff backwards, so long, till the concave surface of the cone received the fundus uteri, and now he lifted the uterus forwards by the handle of the instrument, he held in his right hand. The uterus instantly and with some noise jumped into its normal situation, into the upper pelvic aperture. The prolapsus vaginae, the dysury and the pain instantly ceased, and the pregnancy was normally finished. The author also recommends this instrument which he calls Hysteromochlion or Vectis uterinus in obliquitas, prolapsus and inversio uteri.

Method of Reducing Hernia in Russia.

JOHN CONRAD HILTEBRANTD *on a kind of large dry cupping machine, used by the Russians for reposing incarcerated ruptures.* They take a pot capable of holding about a few pounds of liquid, stop up the hole it has got at the bottom with a cork, rarify the air contained in it by lighted tow, and put it along with the burning tow upon the abdomen, previously rubbed with oil or soap. Thus the abdominal parietes and bowels are, not without pain, drawn into the pot, and the parts contained in the rupture into the abdomen. The pot is removed by drawing the cork, and if the effect is not yet complete, the pot is again replaced. This popular remedy, the author, like many other physicians, has found very efficacious and harmless. He cured with it incarcerations where vomiting and singultus had already taken place, where the pulse was quick, hard, and small; and where the operation was going to be resorted to, other remedies having been found ineffectual. In an inflammatory state, blood should be drawn first; in lusty, dropsical, or pregnant people, this method finds no application. The common Russians, however, make also use of it in childbed, hæmorrhages, and spasms, which, according to their theory, they derive from an irregular position of the uterus.

An Essay on the Oopas, or Poison Tree of Java, addressed to the honourable Thomas Stamford Raffles, lieutenant-governor. By Thomas Horsfield, M. D. (Communicated to the Society by the President.)

(Concluded from page 80.)

Exper. 15.—Having, by the assistance of the commandant of Banjoowangee, obtained from the island of Bali an arrow, supposed to be armed with the oopas from Borneo, I wounded a dog

in the muscles of the thigh. On the 10th minute he became restless, attempted to extricate himself, and barked; in 14 minutes he extended his tongue, had an increased flow of saliva, shewed a disposition to vomit. In 15 minutes he was very much agitated, jumping, barking, and making violent efforts to escape; the attempts to vomit became more repeated. In 25 minutes he appeared exhausted, and extended his limbs. In 30 minutes the muscles of the abdomen were contracted. In 32 minutes he vomited. In 37 he vomited an excremental matter. In 40 minutes he breathed heavily and laboriously; the muscles acted violently. In 45 minutes lying exhausted and breathing hastily. In 50 minutes he started suddenly and barked. In 55 minutes he cried out violently, and having discharged his excrement, after a few interrupted respirations, he died. On dissection the same appearances were observed as after the above related experiments.

Exper. 16.—I obtained a small quantity of the oopas of the island of Borneo, which having moistened, and rendered somewhat fluid with cold water, I applied to a dart, and wounded a dog in the usual manner. The first three minutes he appeared little affected by the wound. On the 5th minute he shewed symptoms of drowsiness, which gradually increased. In six minutes he staggered and reeled round. In 10 minutes the drowsiness returned, after which he reeled round again. He now had an increased flow of saliva, and his breathing became quicker. In 12 minutes he reeled round again, with more violence, and trembled. On the 14th minute he fell down with violent tremors, and extended his extremities convulsed. After a short calm, the symptoms recurred with greater violence on the 15th minute, when, after violent tremors, convulsion, and screaming, he died.

A creeping undulatory motion was observed in the skin after death, over the surface of the whole body, in this and several other instances.

Exper. 17.—The following experiment was made at Soorakarta (in the course of the month of March, 1812) with the poison of the antshar, which I collected at Banjoowangee, in July, 1806. A dog of middling size was wounded in the usual manner in the muscles of the thigh with a dart that had been dipped into the

poison about 24 hours before, and during the interval had been exposed to the open air of a chamber. During the first 20 minutes after the puncture he remained quiet, and shewed few symptoms of uneasiness except a kind of heaviness and fatigue. On the 20th minute his abdominal muscles were somewhat contracted, and he breathed heavier; in 25 minutes he had an increased flow of saliva, and licked his jaws. In 27 minutes he started, screamed violently, fell down convulsed, and discharged the contents of his rectum. On the 28th minute the convulsions returned violently, and continued without interruption till the 30th minute, when he died.

The dissection agrees with those previously made. The stomach was distended: it contained the food previously taken, the poison having acted with uncommon violence, it was not ejected as usual. In the thorax the large vessels were very much distended with blood, exhibiting the appearances above described.

The vessels of the lungs were distended, and the lungs were florid.

On removing the cranium, the brain and dura mater were found nearly natural, the former pale, and perhaps more watery than usual.

SECOND EXPERIMENTS WITH THE TSHETTIK.

Exper. 18.—A dog of middling size was wounded in the muscles of the thigh with a dart covered with the fresh prepared poison of tshettik. In two minutes he shewed symptoms of uneasiness: he appeared faint and lay down. In three minutes and a half he was seized with convulsive twitchings of the extremities, was very restless, and his breathing became quick; these symptoms gradually increasing to the sixth minute, while he continued as exhausted in a lying posture. He now raised himself, extended his head as if attempting to leap, but fell down, was seized with violent convulsions, attended by quick and interrupted breathing to the ninth minute, when he died.

Exper. 19.—A small dog was wounded in the usual manner in the muscles of the thigh with the poison of tshettik. He imme-

diately placed himself in a drooping posture, his fore legs bent as in kneeling, and thus he continued to the 5th minute: he was now seized with trembling, which continued about half a minute, when he suddenly started, extended his head and neck, stretched out his extremities, and, falling on his side, was violently convulsed. His legs continued stiff, extended, and trembling. These symptoms continued with great force until the 8th minute, when they gradually diminished: his respiration became interrupted: he had occasional twitchings to the 11th minute, when he died quietly.

On dissection the contents of the abdomen were found perfectly natural: the stomach was distended with food newly taken in. In the thorax the heart and lungs appeared natural. The aorta was almost empty; and, on being punctured, a small quantity of blood ran out, of a dark colour: the ascending and descending venæ cavæ were distended with dark blood, which, being let out, soon coagulated in the cavity of the thorax. The brain was most affected; the vessels were distended and inflamed; the sinuses were filled with dark coloured blood.

Exper. 20.—A fowl nearly full grown was pierced through the muscles of the thigh with an arrow armed with tshettik. After the first impression was over, it seemed insensible to the wound about one minute, walking round and picking up grains as usual; near the second minute it became giddy, and unable to stand, placed itself into a half sitting posture. On the third minute it began to breathe hastily. In five minutes it trembled, and discharged the contents of its bowels. It now made an attempt to rise, and extended its head and neck; but being unable to support itself, reeled round, fell down, had violent convulsions, with quick interrupted breathing, which continued to the ninth minute, when it died.

Exper. 21.—A fowl was wounded with a poisoned dart in the back near the left wing, the puncture extending towards the cavity of the thorax. In less than one minute it shewed some uneasiness, and could with difficulty support itself. In one minute and a half it had a fluid discharge from the bowels; after which it suddenly started, extended its head and legs, and trembled violently, fluttering with the wings. On the third minute it made a

sudden effort to run, and extended its neck, but fell down head foremost, and was violently convulsed, fluttering with the wings; the respiration was extremely laborious, and soon became interrupted; the convulsions continued to the fourth minute, when it died.

Exper. 22.—A fowl was wounded in the usual manner with an arrow covered with the oopas of tshettik, which had not been mixed with the spices employed in the preparation. On the fortieth second it felt the operation pricking its breast violently, as if it perceived an itching. In one minute it reeled round. In one minute and a half it extended its neck, fell down forwards, fluttered, and was seized with convulsions, which continued to the third minute, when it died.

Exper. 23.—The following experiment was made in August, 1808, two years after the preparation of the poison. A fowl was wounded in the usual manner with a poisoned dart. It died with the above related symptoms two minutes after the experiment.

Exper. 24.—I infused a small portion of the bark of the tshettik in alcohol; having macerated it a few days, I exposed it to the open air for co-operation, and obtained a small quantity of an elegant brown shining resin. A dart was covered with a few grains of this, and a fowl wounded in the usual manner. The first three minutes after the puncture, it remained quiet, and appeared drooping. On the fourth minute it reeled backward, tottered, and its limbs were relaxed. On the sixth minute it appeared to be sleepy, but its drowsiness was frequently interrupted by twitchings, but soon became quiet, and its breathing quick. On the seventeenth minute it had occasional twitchings in the extremities, and was unable to stand erect. On the twentieth minute the drowsiness had considerably diminished; it rose and supported itself, but tottered in attempting to walk. From the thirtieth minute it began to revive; all the effects gradually went off, and on the sixtieth minute it was apparently well.

Exper. 25.—The following experiment was made at Soorakarta, in the month of March of the present year, 1812, nearly six years after the collection of the oopas in Blambangan. A dog of middling size was wounded in the muscles of the thigh with a dart, which,

having been dipped into the oopas, was exposed half an hour to the open air, to give the poison time to become dry. During the first two minutes he stood quiet, and in his appearance only exhibited the pain produced by the wound. On the third minute he was drowsy. In five minutes he began to tremble violently, and reel. On the seventh minute he fell down head foremost, and was convulsed, his extremities being stiffly extended: unable to raise himself again, the convulsions continued with excessive violence till the ninth minute, when he died.

On dissection, his stomach was found natural, and contained the food lately taken in: all the viscera of the abdomen were also natural. In the thorax the venæ cavæ were found completely filled, and the aorta partially filled with blood; the lungs still retained a florid colour. On removing the cranium and exposing the brain, the whole surface of the dura mater was found inflamed, and the vessels were injected with blood; that part covering the right lobe, in particular, was in a state of the highest inflammation; it exhibited externally a livid bluish colour: on the internal surface (of the dura mater) the fluid had been forced out of the vessels by the violence of the action, and it was covered by a bloody lymph. The integuments of the cerebellum were also strongly affected. In the vessels of the surface of the brain itself some marks of inflammation were also perceived. On tracing the wound no evident marks of inflammation appeared, and the remains of the adhering poison were evident along its course.

Exper. 26.—(To show the effect of poison taken *internally.*) To a nearly full grown dog, about half the quantity of poison generally adhering to a dart was given with a little boiled rice. During the first ten minutes he remained quiet, and appeared a little drowsy; on the 14th minute he could with difficulty support himself erect, and indicated symptoms of pain; he showed some disposition to vomit, and extended his jaws. In 28 minutes he extended his hind legs, spasmodical. In 31 minutes he had violent spasms over his whole frame. In 37 minutes he stood breathing hastily, his abdomen appeared uneasy. In 39 minutes, he had spasmodic extensions of his extremities, which lasted half a minute, when he became quiet; but being faint, supported himself

against a wall. In 46 minutes he started up convulsed. In 48 minutes he appeared oppressed in the head and drowsy. In 54 minutes he started up suddenly. In 60 minutes he appeared oppressed and drowsy. In 61 minutes he fell backwards in violent convulsions, his extremities strongly contracted by spasms, after which he became calm. On the 63d. minute, being roused, and attempting to walk, he fell backwards with violent spasms and convulsions. In 65 minutes, having raised himself with difficulty, he stood with his extremities far extended, and his muscles in a state of spasmodic contraction. In 67 minutes he fell down head foremost, violently convulsed, his breathing became interrupted, and on the 69th minute he died.

Dissection.—On opening the abdomen several ounces of clear serous fluid, mixed with streaks of newly coagulated blood, were found effused in the cavity ; the vessels of the external coats of the stomach, of the intestines and mesentery, were in the highest possible degree inflamed and distended beyond their natural size, having evidently been acted on by the most violent force; the stomach being opened, was found empty, its internal coat was corrugated and covered with frothy mucus, in which was found the remains of the poison, a dark yellow fluid with some grains of the rice with which it was conveyed. In the thorax the lungs were still florid, the venæ cavæ much distended, the aorta nearly empty; being punctured, the blood flowed out of a dark hue.

On exposing to view the brain, the dura mater was nearly natural, only the larger vessels somewhat more distended than usual; the vessels of the brain itself indicated a slight degree of inflammation.

Remarks on the experiments.—I have selected, from a large number of experiments, those only which are particularly demonstrative of the effects of the antshar and of the tshettik, when introduced into the circulation. The poison was always applied by a pointed dart or arrow, made of bamboo. The extremity to which the poison adhered was completely spear-shaped, about an inch long, and a line and a half broad near the middle of its length.

When I contemplated an experiment, the dart was dipped into the fluid poison, which I preserve in closed vessels. It is neces-

sary to give it some time to dry, and fixed upon the dart. I found by repeated trials the poison most active after having adhered twenty-four hours to the weapon; if applied in a fluid state, it does not enter the wound in a sufficient quantity to produce its effects; but in the attempt to thrust it through the muscles, it separates itself from the dart, and adheres externally to the integuments.

The operation of the two different poisons on the animal system is essentially different.

The first seventeen experiments were made with antshar; the rapidity of its effects depends in a great degree on the size of the vessels wounded, and on the quantity of poison carried into the circulation.

In the first experiment it induced death in 26 minutes; in the second, which was made with the sap collected in Poogar, in 13 minutes. The poison from different parts of the island has been found nearly equal in activity.

In the ninth experiment, with the poison from Passooroowang, death followed in 29 minutes.

The common train of symptoms is a trembling and shivering of the extremities, restlessness, erection of the hair, discharges from the bowels, drooping and faintness, slight spasms and convulsions, hasty breathing, and increased flow of saliva, spasmodic construction of the pectoral and abdominal vessels, retching, vomiting, excremental vomiting, frothy vomiting, great agony, laborious breathing, violent and repeated convulsions, and death.

The effects are nearly the same on quadrupeds, in whatever part of the body the wound is made. It sometimes acts with so much force that not all the symptoms enumerated are observed; in these cases after promonitory symptoms (tremors, twitchings, faintness, and an increased flow of saliva) the convulsions come on suddenly, and are quickly followed by death. See the 17th experiment.

The oopas appears to affect different quadrupeds with nearly equal force, proportionate in some degree to their size and disposition. To dogs it proved mortal, in most experiments, within an hour; a mouse died in ten minutes, see experiment 8th; a mon-

key in seven minutes, see experiment 11th; a cat in fifteen minutes, see experiment 12th.

A buffalo, one of the largest quadrupeds of the island, died in two hours and ten minutes, see experiment 13th. I do not think the quantity of poison introduced in this experiment was proportioned to that which was thrown into the system in the experiments on smaller animals; the dart fell from the wound before a sufficient quantity had been taken into the circulation to produce a rapid effect. If an animal is pierced by an iron spear to which the poison has been applied, it shows comparatively but little of the effects, because the weapon is again retracted, and the poison does not remain in contact with the wound long enough to be taken into the circulation. Mr. Leschenaut de la Tour stabbed a buffalo a number of times successively with a common spear or pike of the Javanese, largely covered with the poison of the tshettik, without very sensibly affecting the animal. A dart or arrow prepared of bamboo is a more fit instrument to introduce the oopas; having once pierced the skin it easily adheres to the parts it comes in contact with, on account of its inconsiderable weight.

The natives of Macasser, Borneo, and the eastern islands, when they employ this poison, make use of an arrow of bamboo, to the end of which they attach a shark's tooth, which they throw from a blow-pipe or *sompit*.

The 15th and 16th experiments are *comparative;* they were made with the oopas from Bali and Borneo; by contrasting them with the first, second, ninth, and seventeenth experiments, it sufficiently appears how far the oopas of the different islands agrees in activity. It is probable that the oopas from Borneo, when fresh, may act more forcibly than that of Java.

If the simple or unprepared sap is mixed with the extract of tobacco or stramonium, (instead of the spices mentioned in the account of preparation,) it is rendered equally, perhaps more active. See the 3d and 4th experiments.

Even the pure juice, unmixed and unprepared, appears to act with a force equal to that which has undergone the preparative process, according to the manner of the Javanese at Blambangan. See the 5th experiment made with the fresh juice of Ban-

jaswangee, and the 10th experiment with the fresh juice collected at Goorrong, near Passooroowang.

Birds are very differently affected by this poison; fowls have a very peculiar capacity to resist its effects. In the 44th experiment, a fowl died 24 hours after the wound, others have recovered after being partially affected.

The 6th and 7th experiments show the effects of the unprepared juice on two birds of the genus Ardea.

The 18th and the succeeding experiments were made with the poison prepared from the tshettik. Its operation is far more violent and rapid than that of the antshar, and it affects the animal system in a different manner;—while the antshar operates chiefly on the stomach and alimentary canal, the respiration and circulation, the tshettik is determined to the brain and nervous system.

A relative comparison of the appearances on dissection demonstrates in a striking manner the peculiar operation of each.

The 18th, 19th, and 25th experiments give a general view of the effects of the tshettik on quadrupeds.

After the previous symptoms of faintness, drowsiness, and slight convulsions, it acts by a sudden impulse, which, like a violent apoplexy, prostrates at once the whole nervous system.

In the 18th and 19th experiments this sudden effect took place on the sixth minute after the wound, and in the 25th experiment, on the seventh minute, the animals suddenly started, fell down head foremost, and continued in convulsions till death ensued.

This poison affects fowls in a much more violent manner than that of the antshar, as appears from the 20th and 21st experiments; they are first affected by a heat and itching of the breast and wings, which they show by violently picking these parts; this is followed by a loose discharge from the bowels, when they are seized with tremors and fluttering of the wings, which having continued a short time, they fall down head foremost, and continue convulsed till death. I have related such experiments as show the gradual operation of the poison; in some instances (especially in young fowls) it acts with far greater rapidity; death has

frequently occurred within the space of a minute after the puncture with a poisoned dart.

It appears from the 22d experiment, that the simple unmixed decoction of the bark of the root of the tshettik is nearly as active as the poison prepared according to the process above related.

The 24th experiment shows plainly, that the resinous portion of the bark is by no means so active as the particles soluble in water; a fowl wounded by a dart covered with the pure resin recovered after being very partially affected; it has also been remarked above, that, in the preparation of the dried juice of the antshar, the resinous parts are thrown away. The strength of the poison remains unimpaired, if carefully preserved, a number of years, as is evident from the experiments made at different periods of its age.

Taken into the stomach of quadrupeds, the tshettik likewise acts as a violent poison, but it requires about twice the period to produce the same effect which a wound produces.

In the 26th experiment its operation internally is detailed, and the appearances after death are described in the account of the dissection.

But the stomach of fowls can resist its operation; having mixed about double the quantity generally adhering to a dart with the food of a fowl, it consumed it without having any marks of indisposition.

The poison of the antshar does by no means act so violently on quadrupeds as that of the tshettik. I have given it to a dog. It produced at first nearly the same symptoms as a puncture; oppression of the head, twitchings, faintness, laborious respiration, violent contraction of the pectoral and abdominal muscles, an increased flow of saliva, vomiting, great restlessness, and agony, &c. which continued nearly two hours; but after the complete evacuation of the stomach by vomiting, the animal gradually recovered.

Rumphius goes so far as to assert, that a small quantity may be taken internally as a medicine. In speaking of the qualities of the arbor toxicaria, he says, the crude and unmixed ipo is an antidote to the bite or sting of venomous fishes and insects; also, that

a person affected by an eruption of the skin or vacuations, may take a small pill of the oopas, which will attract all impurities from the intestines and carry them off.

The appearances observed on dissection explain, in a great degree, the relative operation of the poisons. In animals killed by the antshar, the large vessels in the thorax, the aorta, and venæ cavæ, were in every instance found in an excessive degree of distention. The increase in the vicinity of the source of circulation, especially the lungs, were uniformly filled in a preternatural degree with blood, which, in this viscus and in the aorta, still retained a florid colour, and was completely oxygenated. On puncturing these vessels, it bounded out with the elasticity and spring of life. The vessels of the liver, of the stomach, and intestines, and of the viscera of the abdomen in general, were also more than naturally distended, but not in the same degree as those of the breast. In the cavity of the abdomen a small quantity of serum was sometimes effused.

The stomach was always distended with air, and in these instances, in which the action of the poison was gradual, and in which vomiting supervened in the course of the symptoms, its internal coat was covered with froth.

The brain indicated less of the action of the poison than the viscera of the thorax and abdomen. In some instances it was perfectly natural, in others marks of a small degree of inflammation were discovered.

An undulatory motion of the skin, and of the divided muscles, was very evident in some of the dissected animals.

The appearances observed in the animals destroyed by the tshettik, were very different. In a number of dissections the viscera of the thorax and abdomen were found nearly in a natural state, and the large vessels of the thorax exhibited that condition in which they are usually found after death from other poisons.

But the brain and the dura matter showed marks of a most violent and excessive affection. In some instances the inflammation and redness of the dura matter was so strong, that on first inspection, I supposed it to be the consequence of a blow previously

received, until I was taught by repeated examinations, that this is an universal appearance after death from tshettik.

I am not at present at leisure, nor am I properly prepared to investigate fully the operation of the two poisons described on the animal system, or to elucidate their effects by a comparison with other poisons. The series of experiments I have proposed to myself, and which are necessary for the purpose, is by no means finished, nor does my situation at present afford me those opportunities of scientific consultation, which such an investigation requires; it remains for a future period, also, to determine, relatively, the force of these poisons with that of the most venomous serpents; the tshettik exceeds, perhaps, in violence, any poison hitherto known. It shows its effects peculiarly, and almost exclusively, on the brain and nervous system.

The action of the antshar is directed chiefly to the vascular system. The volume of the blood is accumulated in a preternatural degree in the large vessels of the thorax.

The circulation appears to be abstracted from the extremities, and thrown upon the viscera near its source. The lungs in particular are stimulated to excessive excretions. The balance of circulation is destroyed. The vital viscera are oppressed by an intolerable load, which produces the symptoms above described, while in the extremities a proportionate degree of torpor takes place, accompanied by tremors, shivering, and convulsions.

I have but little to add concerning the operation of the antshar on the human system. The only credible information on this subject is contained in the work of Rumphius, who had an opportunity of personally observing the effect of the poisoned darts or arrows, as they were used by the natives of Macassar, in their attack on Amboina, about the year 1650.

They were also employed by the inhabitants of Celeber, in their former wars with the Dutch. Speaking of their operation, he says, the poison, touching the warm blood, is instantly carried through the whole body, so that it may be felt in all the viens, and causes an excessive burning, and violent turning in the head, which is followed by fainting and death.

The poison (according to the same author) possesses different

degrees of violence, according to its age and state of preservation.

The most powerful is called oopas radja, and its effects are considered as incurable; the other kinds are distributed among the soldiers on going to war. After having proved mortal to many of the Dutch soldiers in Amboina and Macassar, they finally discovered an almost infallible remedy in the root of the crinnum asiaticum, (called by Rumphius, radix toxicaria,) which, if timely applied, counteracted by its violent emetic effect the force of the oopas.

An intelligent Javanese, at Banjoowangee, informed me, that, a number of years ago, an inhabitant of that district was wounded in a clandestine manner by an arrow thrown from a blow-pipe, in the fore-arm, near the articulation of the elbow. In about fifteen minutes he became drowsy, after which he was seized with vomiting, became delirious, and in less than half an hour he died.

From the experiments above related on different quadrupeds, we may form an analogous estimate of its probable effects on man.

A case of Extra-Uterine Fœtus, contained in the Fallopian Tube, with some Observations. By George Langstaff, Esq. (Read June 24th, 1817.)

[From the Dublin Hospital Reports.]

In the seventh volume of the Society's Transactions, I related a case of an embryo, with some of its involucra, situated in the fallopian tube; which occasioned the death of the mother by hæmorrhage from laceration of that tube. The peculiarities were obliteration of the canal, from where the ovum was, to the uterus; and the latter not having prepared a deciduous membrane. Since the publication of this case, I have had an opportunity of examining another, where the reverse of this took place; the fallopian tube remaining open, and a decidua uteri having been completely formed.

Although it has been said, that extra-uterine fœtuses are not unfrequent, and that the relation of such cases is not of any practical utility, and does not tend to throw light on the physiology of conception; yet I feel great satisfaction in exhibiting to the society the preparations of those facts, accompanied with as brief a history of the last case and dissection as I can give. Should it be considered by the council deserving a place in the Transactions, I shall feel amply repaid for my trouble.

Case.—A healthy woman, thirty years of age, (a patient of my friend Mr. Snow, surgeon, Highgate,) was suddenly attacked on the 14th of April, 1817, with excruciating pain in the lower part of the abdomen and right groin, succeeded by almost constant distressing vomiting; which continued sixteen hours, without the least mitigation, although a variety of remedies were employed. After the cessation of pain and vomiting, the patient became extremely restless and anxious; her pale countenance, and small quick pulse, denoted considerable internal mischief; and the most probable conjecture respecting the nature of the complaint was, that some large blood-vessel had been ruptured. The abdomen enlarged, and occasioned dyspnœa; the vital powers gradually yielded, and she expired forty-eight hours from the commencement of the attack.

Dissection.—The viscera of the abdomen were completely covered with blood, partly fluid and partly coagulated; and there was a considerable quantity in the pelvis. The whole nearly filled two moderate sized wash-hand basins.

The pelvic viscera were removed, and the cause of death was found to be a rupture of the right fallopian tube in consequence of a fœtus and its membranes having been detained there instead of passing on to the uterus. The ovum was situated about half an inch from the fimbriated orifice of the tube. This canal was greatly enlarged by the ovum, and the part which held it was rendered extremely thin, more especially at its posterior surface, where it had burst to the extent of half an inch.

The exact age of the embryo cannot be stated, as the mother's

menstrual periods had not been interrupted by this impregnation; but from its size I should have supposed it to have been between the sixth and seventh week. The corpus luteum in the right ovarium was very large; and in the left there was the sign of a prior impregnation.

The uterus was somewhat enlarged, and it contained a most beautiful and perfectly formed decidua; but the os uteri was not closed, nor were the mucous glands distended as in the case I first described.

On the left side, the fimbriated part of the fallopian tube had formed adhesions to the posterior surface of the peritoneal covering of the uterus and the ovarium; so as to have completely precluded, during the person's life, the exact application of those parts, which is essential to generation.

Observations.—The above-mentioned appendages of the uterus were not brought into close union, but connected by very delicate longish layers of organized lymph, arranged in a reticulated manner. Such appearances I have frequently noticed on one or both sides of the uterine appendages, more especially in Cyprians; which perhaps may have been occasioned by frequent excessive excitement. I have also seen similar effects in married women, who have not been prolific; and from those facts I have no hesitation in saying, that such adhesions form the principal causes of barrenness. I possess several preparations, showing the variety of ways in which the fimbriated extremities are prevented from embracing the ovaria, by those simple morbid productions of coagulated lymph. In some, the morsus diaboli, as they have been called, adhere to the anterior, or posterior surface of the ligamenta lata, or uterus. In others, bands of adhesions are formed between the fallopian tubes, and that part of the peritoneum reflected from the uterus over the rectum; or are seen adhering to that portion which covers the posterior surface of the bladder. I have occasionally found the mouths of the fallopian tubes obliterated.

[From the New-York Evening Post, Sept. 1, 1818.]

The following letter cannot be read without great interest by our medical men and naturalists.

Paris, June 1.

DEAR SIR,

I now transmit to you several late journals and medical publications, in which much interesting matter is offered for philosophical enquiry. The sudden change of the colour of the skin from white to black, in the person of *Mary Galliard*, is truly astonishing, and invites new conjectures and controversies. The reporter of the case to the faculty of medicine, has attempted to explain this phenomenon, from a disease of the liver and from bile; but, I think, that neither have any thing to do with it. I have seen the woman before she died, and it is a fact that her change of colour took place in the night after she was informed of the tragical death of her children. It is also true that she continued eighteen months in the same colour, and until she died; but she was observed to grow paler half an hour before her body was opened. I hope you will not leave the subject without farther inquiry. Whoever could satisfactorily account for this strange metamorphosis, would certainly explain or disclose the true cause of the black colour of the African race.

You will not be less astonished by the operation for cancer, as performed by Richerand. The sufferer, who was a surgeon himself, had several times submitted to the entire extirpation of his cancer in the left breast, also to several applications of fire and caustic, but it always shot out anew, and put forth more horrid excrescences and offensive discharges. No chance could be left against an impending and cruel death, but from the excision of two ribs, the *plura* underneath being found much diseased, it was also cut off on a quadrilateral space of eight inches square. What terrible consequences were now to be remedied, of hæmorrhage from the intercostal arteries, and of suffocation by the air

rushing into the lungs, any man almost can be aware of: but they were all admirably and successfully provided for. ————On the twenty-seventh day after the operation, the patient was perfectly cured, retaining a leathery plate on the scar, being yet tender.

Richerand exults now for having proved to the world, that, for very important purposes, the cavity of the thorax may be opened by excision of the ribs and of the pleura; in case of a great lesion of a lobe of the lungs, a part may be cut off; and by hydropericardium might be operated as an hydrocele.

It was discovered by this operation, that the heart and pericardium are absolutely insensible, the last being so transparent as to show all the motions of the former, that like the mirror of the eye, it becomes opaque only by death.

Remaining, dear sir, yours truly, &c. L. V.

To Pelix Pascalis, M. D. Corresponding Member of the Society and Faculty of Medicine of Paris, &c.

Interesting Discovery.

Dr. Baldwin, late surgeon of the frigate Congress, has decided the controversy respecting the habitat of the potato, solanum tuberosum. He found this vegetable growing abundantly on the north side of the Rio de la Plata, in wild uncultivated situations, unknown to the inhabitants, who do not even cultivate this valuable plant, now so generally attended to in most parts of the civilized world.

It is found growing among the rocks on Monte Video, and in the vicinities of Maldenado, in the sand hills on the river shore, as well as in low moist situations, near streams of water. The largest tubers were not more than half an inch in diameter.

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TO CORRESPONDENTS.

SEVERAL interesting communications for the Recorder came to hand too late for this number: They shall appear in our next.

TO SUBSCRIBERS.

As the first volume of the Recorder is now completed, the publisher respectfully solicits the subscribers to make payment as early as possible.

ERRATA.

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—	167	— 14	for Ferrier, read Ferriar.
—	168	— 12	for house, read home.
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—	169	— 2	for course, read source.
—	—	— 13	for those, read these.
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—	—	— 7 from below,	for agitation, read action.
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